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ARTICLES

PARTISAN CYCLES IN CONGRESSIONAL ELECTIONS AND THE MACROECONOMY

ALBERTO ALESINA
Harvard University

HOWARD ROSENTHAL
Carnegie-Mellon University

In the postwar United States the president's party has always done worse in the midterm congressional elections than in the previous congressional election. Republican administrations exhibit below-average, and Democratic administrations above-average, economic growth in the first half of each term, whereas in the latter halves the two see equal growth. Our rational expectations model is consistent with these two regularities. In presidential elections, voters choose between two polarized candidates. They then use midterm elections to counterbalance the president's policies by strengthening the opposition in Congress. Since presidents of different parties are associated with different policies, our model predicts a (spurious) correlation between the state of the economy and elections. Our predictions contrast with those of retrospective voting models, in which voters reward the incumbent if the economy is doing well before the election. Our model performs empirically at least as well as, and often better than, alternative models.

Macroeconomics and macropolitics are deeply interconnected. In the years since the conclusion of the Second World War in the United States there have been some strong empirical regularities in both the economic and political arenas. A political regularity, the midterm congressional cycle, is well known. As shown in Figure 1, the party holding the White House has lost vote share in midterm elections both in the House and in the Senate. The figure shows the Republican percentage of the two-party vote. For the intervals from "on" to "off" years (1948–50, 1952–54, etc.), the curves always slope downwards with a Republican in office and upwards with a Democratic president.

A macroeconomic regularity was first noted by Hibbs (1977), who showed that unemployment tended to fall when the Democrats occupied the White House and rose on a Republican watch.[1] Alesina and Sachs (1988), and Alesina (1988b) refined this observation, by showing that real

Figure 1. The Midterm Election Cycle: Republican Percent Two-party Vote

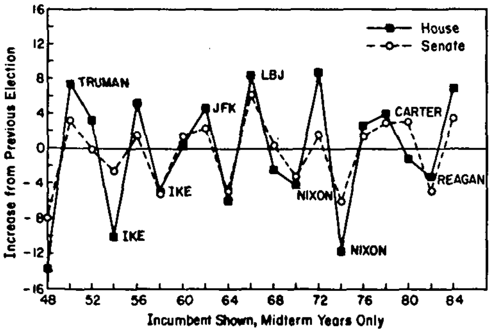

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Table 1. Rate of Growth of Real GNP

Administrations	Years			
	First	Second	Third	Fourth
Democratic				
Truman	.0	8.5	10.3	3.9
Kennedy/Johnson	2.6	5.3	4.1	5.3
Johnson	5.8	5.8	2.9	4.1
Carter	4.7	5.3	2.5	-.2[a]
Average	3.3	6.2	5.0	3.3
Average 1st/2d halves	4.8		4.1	
Republican				
Eisenhower I	4.0	-1.3	5.6	2.1
Eisenhower II	1.7	-0.8	5.8	2.2
Nixon	2.4	-0.3	2.8	5.0
Nixon/Ford	5.2	-0.5	-1.3[a]	4.9
Reagan I	1.9	-2.5	3.6	6.4
Reagan II	2.7	2.5	—	—
Average	3.0	-0.5	3.3	4.1
Average 1st/2d halves	1.2		3.7	

Source: Economic Report of the President 1987 (Washington: GPO, 1988).
[a]Oil shocks.

gross national product tends to increase at above-average rates in the first two years of Democratic administrations and increase at below-average rates in the first two years of Republican administrations, while the second halves of the two administrations show little if any difference (see Table 1). Similar results hold for unemployment (Chappell and Keech [1988] and Alesina [1988b]). In particular, five of the seven postwar recessions have started soon after a Republican president was elected. Complementing these results on output and unemployment, several researchers—including Alesina (1988b), Alesina and Sachs (1988), Beck (1982, 1984a), Chappell and Keech (1988), Havrilesky (1987), Hibbs (1987); and Tabellini and La Via (n.d.)—find that monetary policy has been "looser"—thus more inflationary—when a Democrat rather than a Republican was in the White House.

We present a model consistent with these two regularities. Individual agents in both the economy and the electorate are fully rational and forward looking, as in Chappell 1983 and Chappell and Keech 1985. Frictions in the economy and the political process generate the observed cyclical behavior. The political friction is that candidates have polarized policy preferences. We treat these preferences as exogenous and do not model them. The presence of polarization during the postwar period is supported not only by casual observation but also by empirical studies of voter evaluations of presidential candidates (Poole and Rosenthal 1984b), of interest group evaluations of members of Congress (Poole and Daniels 1985; Poole and Rosenthal 1984a), and of roll call voting patterns (Poole and Rosenthal 1985a, 1985b). Democrats have a higher tolerance for inflation and a lower tolerance for unemployment than do Republicans. Because the outcome of elections cannot be fully anticipated, these policy preferences will lead to expectation uncertainty.

The economic friction is that plans and contracts made prior to elections cannot be immediately revised once the winner is known. In addition, in the model, economic agents cannot enter into state-contingent contracts that provide insurance against electoral risk. Specifically, some wage contracts are signed while the inflation rate that will occur after the election is still unknown, since there is electoral uncertainty. Thus, even a rational public cannot predict with certainty the postelection inflation rate. This generates a short-run Phillips curve, even in a rational expectations model. However, since contracts and plans can be reformulated after the election, the real effects diminish over time and eventually disappear. This is consistent with the observation that the real effects on output and unemployment are largely concentrated at the beginning of each administration.

The inability to anticipate elections also affects the behavior of pivotal voters, who in our model are moderate relative to either party. They would like policies that stood between those pursued by Democratic and Republican presidents. Since we assume that actual policy is unidimensional and a function of the preferences of the executive and of the relative strength of the two parties in Congress, pivotal voters can use their congressional votes to moderate the outcomes of presidential elections.

Some of this moderating behavior may take place in presidential years, consistent with the empirical observation of massive split ticket voting in the U.S. electorate. In midterm elections this effort to counterbalance the president's policies should continue. Thus, there will be a shift in allegiance of some voters who supported the president's party during the presidential election. This shift generates the midterm electoral cycle. Our model does not consider coattails and other explanations of the midterm cycle. Instead, our purpose is to present a model consistent with both these voting regularities and the regularity concerning the macroeconomy.

We eliminate other potential frictions in order to make the model tractable. Specifically, we assume that (1) there are no lagged effects in the polity or the economy and (2) voters are perfectly informed about the preferences of the policy makers and about the state of the economy. These assumptions eliminate any incumbency effect. In addition, since macroeconomic variables depend only on current government policy and agents' expectations of those policies, there is no carryover from past policies. Consequently, we can examine each presidential election and the ensuing congressional election as a two-period game, isolated from past and future history. Alesina and Tabellini (1987) and McKibbin, Roubini, and Sachs (1987) consider models with economic state variables that link current to future administrations.

Matters are further simplified—both for tractability and to highlight the key elements of our model—by assuming that the only source of uncertainty is about the distribution of voter preferences. Thus we have eliminated several important sources of uncertainty. For one, preferences of presidential candidates are assumed known prior to an election. For another, there are no exogenous shocks to the economy. As a result, voters do not need to learn anything about the preferences or competence of politicians from observing the state of the economy. For models with learning and asymmetric information, see Alesina and Cukierman 1987, Cukierman and Meltzer 1986, Rogoff 1987, and Rogoff and Sibert 1988. If learning takes place, we would find retrospective, as well as moderating, voting. In addition to restricting the form of uncertainty, we also later make further simplifying assumptions for technical reasons. We conjecture that the qualitative nature of our results holds in more general specifications.

The predictions of our model contrast sharply with those of other politico-economic models. Nordhaus (1975) suggested that administrations faced nonrational agents and could therefore create an expansion immediately before elections. This "electoralist" policy would be followed by both parties, since they both care only about reelection. McCallum (1978)—and several others after him—provided a substantial body of empirical results rejecting the Nordhaus model.[2] An alternative to it is provided by Hibbs's "partisan" cycle. Hibbs's work, however, remains similar to Nordhaus's in the assumption of nonrational economic agents, since it is based on a traditional Phillips curve.

On the political side, the major alternative to our model is the retrospective voting formulation introduced by Kramer (1971). (See also Fiorina [1981].) In retrospective models, voters vote for the incumbent president's party in good times and go against it in bad times. Moreover, as argued in most detail by Fair (1978, 1982, n.d.), the data would suggest that voters have short memories. Only the state of the economy in the six months prior to the election has any bearing on the election results. In contrast, in the model we present here, voters vote solely to influence the future of the economy. Since voters have full information about the parties' preferences, past performance is irrelevant in predicting the future. On the other hand, since pivotal voters seek moderation and since the first two years of administrations will lead to fluctuations in output, our model does point to an expected correlation between past economic performance and voting behavior. In "off" years, voting for the Democrats should be negatively correlated with the deviation from the long-term growth rate of the economy. The reason is that years of above-average growth will occur under a Democratic president. When the electorate votes to moderate Democratic presidents, the Democratic congressional vote will decline. In years when the Democrats hold the White House, the prediction of our model contrasts sharply with that of Kramer and Fair. In years of below-average growth—produced by Republican presidents—the two models agree in predicting that the Democrats will do well.

Finally, it should be noted that Fiorina (1988) has independently developed a model of balancing in elections that is related to ours. While Fiorina does not address the midterm effect, he develops several interesting propositions about split ticket voting that are consistent with our model. However, Fiorina suggests that both types of split ticket voting should be observed. Instead, our definition of equilibrium implies that only one type should be observed. An additional difference between the models is that in Fiorina's only four outcomes are possible, while we consider a continuum of possible outcomes.

Because our model leads to predictions different from those of the literature, we present some empirical analysis of aggregate election results since World War II. We find that much previous work, particularly Kramer's, fails to replicate on this period; whereas our model tests out rather well. Our results cover the Senate as well as the House of Representatives. The empirical section is preceded by a formal presentation of our theoretical model. Finally, we offer some concluding remarks.

The Model

As in Alesina 1987 the economy is described by a nominal wage contract model, based on Fischer 1977. In this model wage setters attempt to maintain the real wage at the level compatible with the natural rate of unemployment. Labor contracts last one period and are signed at the end of, say, period $(t - 1)$ for period t. These contracts are noncontingent; in particular, full indexation is excluded. Thus wage setters set the nominal wage growth equal to expected inflation:

$$w_t = \pi_t^e = E(\pi_t | \mathbf{I}_{t-1}), \qquad (1)$$

where w_t = nominal wage growth, π_t = rate of inflation, $E(\bullet)$ is the mathematical expectation operator, and $\mathbf{I}_{t-1}$ is the information set available in period $(t - 1)$. The superscript e indicates rationally expected variables. Given relationship 1, the supply function for this economy can be written as follows:

$$y_t = \gamma(\pi_t - \pi_t^e) + \bar{y}, \quad \text{with } \gamma > 0, \bar{y} \geq 0 \qquad (2)$$

where y_t is the rate of growth of output and $\bar{y}$ is the rate of growth of output compatible with the natural rate of unemployment. We assume that the rate of inflation can be controlled directly by the policy makers.[3]

Electoral competition has the following structure. Every two periods—say, in periods $t = 0, 2, 4, \ldots$—presidential elections are held. In these elections a president and Congress are elected. We refer to *Congress* as the unique legislative body. In nonpresidential elections years (i.e., $t = 1, 3, 5, \ldots$) a new Congress is elected. The two candidates for president are labeled D and R; and since we do not distinguish between *candidates* and *parties*, D and R are used for both interchangeably. The objective functions of the candidates, defined on output and inflation, are given by

$$U^D = -\sum_{t=0}^{\infty} \beta^t[(1/2)(\pi_t - \bar{x}^D)^2 + (b^D/2)(y_t - k^D)^2] \qquad (3)$$

$$U^R = -\sum_{t=0}^{\infty} \beta^t[(1/2)(\pi_t - \bar{x}^R)^2 + (b^R/2)(y_t - k^R)^2], \qquad (4)$$

(The superscripts identify the party.) The difference in the objectives of the two parties can be captured by the following inequalities:

$$\bar{x}^D \geq \bar{x}^R, \; b^D \geq b^R, \text{ and } k^D \geq k^R. \qquad (5)$$

The two parties are not identical if at least one of the three inequalities in number 5 holds strictly. The first highlights a difference in the desired inflation rate, which can be related to a different need for the inflation tax, for instance, because the two parties disagree about the optimal level of government spending. The second and third capture a different evaluation of the costs on inflation and unemployment. In order to simplify the exposition as much as possible we restrict the difference between the two parties' objectives to the first inequality (which we assume strict) and we impose $b^D = b^R = 0$. There is no loss of generality in this simplification because if we substitute relationship 2 into relationships 3 and 4 we would still obtain unidimensional objective functions on inflation, with different optimal and time-consistent policies for the two parties.[4] Thus the objective functions of the two parties are

$$U^D = \sum_{t=0}^{\infty} \beta^t u_t^D = -\sum_{t=0}^{\infty} \beta^t[(1/2)(\pi_t - \bar{x}^D)^2] \qquad (6)$$

$$U^R = \sum_{t=0}^{\infty} \beta^t u_t^R = -\sum_{t=0}^{\infty} \beta^t[(1/2)(\pi_t - \bar{x}^R)^2]. \qquad (7)$$

Voters, like the candidates, have quadratic preferences defined on inflation and know the preferences of the two parties; that is, they know relationships 6 and 7. We assume a continuum of voters in which the distribution of voters' bliss points (x^i), with i denoting the generic voter, is uniform. Without further loss of generality, this distribution is of unit length. Its extremes are given by

$$[a, 1 + a], \qquad (8)$$

where a is a random variable with zero mean-distributed on ($a^{\min}$, $a^{\max}$). We also impose the following inequalities:

$$a^{\max} < \bar{x}^R < \bar{x}^D < 1 + a^{\min}, \qquad (9)$$

which imply that for any realization of a, there are voters with bliss points on both sides of $\bar{x}^R$ and $\bar{x}^D$. The distribution of a and the unit length of the voter distribution are "common knowledge." Since voters' preferences are random, the electoral results are probabilistic even if there is no uncertainty about the policies followed by the two parties if elected.

As a benchmark, consider the case in which Congress has no impact on the inflation policy (or, assume that Congress does not exist). In this case, presidents follow the policies

$$\pi_j^R = \bar{x}^R, \ \pi_j^D = \bar{x}^D \quad j = (t, t + 1). \qquad (10)$$

Voters rationally expect these policies and vote accordingly. In particular, each voter votes for the party with the bliss point closest to his or her own. The probability of electing the Democratic candidate is then given by the probability that the realization of the random variable a is such that more than half of the voters have a bliss point closer to $\bar{x}^D$ than to $\bar{x}^R$. We indicate this value with $\bar{P}$.

In this model the candidates do not converge fully, as in standard median voter models, nor do they partially converge, as in the case of ideologically motivated candidates studied by Wittman (1977, 1983) and Calvert (1985). In fact, as shown in Alesina 1988a, the two presidents cannot commit to any platform other than relationship 10. For instance, if the Democratic candidate could commit to a preelectoral platform he or she would choose a convergent policy, lower than the bliss point, in order to capture (probabilistically) middle-of-the-road voters. However, if precommitments are ruled out, voters know that when in office, the presidents would follow their most preferred policies.[5]

Expected inflation is given by

$$\pi_t^e = \bar{P}\bar{x}^D + (1 - \bar{P})\bar{x}^R, \quad \text{with } t \text{ even} \qquad (11)$$

$$\pi_t^e = \bar{x}^D, \text{ with } t \text{ odd, } D \text{ in office;}$$
$$\pi_t^e = \bar{x}^R, \text{ with } t \text{ odd, } R \text{ in office.} \qquad (12)$$

Relationships 11 and 12 underscore that in the first period of a new administration there is expectation uncertainty because contracts have to be signed before the elections. In the second period expectations are correct since the public has learned the identity, and thus the preferences, of the policy maker in office. Thus there is no uncertainty. The implications of relationship 12 for the output equation are as follows:

$$y_t^D = \gamma(1 - \bar{P})(\bar{x}^D - \bar{x}^R) + \bar{y} \quad \text{if } D \text{ elected in period } t,$$

$$y_t^R = -\gamma\bar{P}(\bar{x}^D - \bar{x}^R) + \bar{y} \quad \text{if } R \text{ elected in period } t,$$

$$\text{and } y_{t+1} = \bar{y} \text{ if } D \text{ or } R \text{ elected in period } t. \qquad (13)$$

Thus a recession occurs in the first half of a Republican administration and an expansion in the first half of a Democratic administration. There is no difference in output in the second halves of the two administrations.

These are the empirical implications

successfully tested by Alesina and Sachs (1988) and Alesina (1988b). We now proceed to consider the role of Congress in this model by assuming that in the second period of a term of office the president is constrained in policy making by the composition of Congress. Thus voters use the midterm election to counterbalance the president in office. In every midterm election the party of the president in office loses votes relative to the preceding election.

We assume that if a Republican president is in office in period $t + 1$ (assuming that presidential elections took place in period t) the inflation policy is given by

$$\pi^R_{t+1} = \bar{x}^R + kV^{DR}, \text{ with } k > 0, \qquad (14)$$

where V^{DR} is the share of votes received by the Democrats in the congressional elections held at the beginning of period $t + 1$. If a Democratic president is in office in period $t + 1$, the inflation policy is given by

$$\pi^D_{t+1} = \bar{x}^D - q(1 - V^{DD}), \text{ with } q > 0, \qquad (15)$$

where V^{DD} is the share of votes received by the Democrats in the congressional elections held at the beginning of period $t + 1$. V^{DR} and V^{DD} will be derived endogenously. For simplicity and with no loss of generality we also assume that

$$\bar{x}^D - q > \bar{x}^R + k. \qquad (16)$$

The linearity of the functions of relationships 14 and 15 helps in solving the model, but it is not crucial for the qualitative features of the results. In other words, the results derived below are not of the "knife edge" type. They hold for other specifications of relationships between V^{DR} and V^{DD} and π^R_{t+1} and π^D_{t+1} in which the curvature of the function is not too far from linearity. Finally, for expositional purposes we assume, for the moment, that in the first period of a term of office the president is unconstrained by the composition of Congress.[6]

The model is solved by backward induction, in order to ensure dynamic consistency. Thus we start from the midterm elections in period $t + 1$. Consider first the case in which a Republican president is in office in period t. We want to find V^{DR} under the assumption that the voters know the effect of the composition of Congress on policy making; that is, they know relationship 14. We make use of the following notion of equilibrium:[7]

DEFINITION. *A voter equilibrium occurs if and only if no voter would prefer a decrease in the expected vote for the party voted for.*

Since voters have single peaked preferences, there exists a cut point in the distribution of voters' bliss points, labeled $\tilde{x}_R$, such that every voter with a bliss point higher than $\tilde{x}_R$ votes for the Democratic party, and vice versa. Given the uniform distribution of number 8, $\tilde{x}_R$ satisfies

$$V^{DR} = 1 + a - \tilde{x}_R. \qquad (17)$$

It is easy to show (by contradiction) that $\tilde{x}_R$ is such that $\bar{x}^D > \tilde{x}_R > \bar{x}^R$.

The expected utility (when a Republican president r is in office) of voter i—with a bliss point x^i such that $\bar{x}^D > x^i > \bar{x}^R$—can be written, using relationships 14 and 17, as

$$EU^{Ri} = \int_{a^{\min}}^{a^{\max}} -(1/2)(\bar{x}^R + k(1 + a - \tilde{x}_R) - x^i)^2 f(a)da. \qquad (18)$$

Note that relationship 18 would not represent the correct expected utility for the voters with bliss points x^j such that $x^j < a^{\max}$ or $x^j > 1 + a^{\min}$. In fact, by knowing both their own preferences and the overall uniform distribution of others' preferences, these voters can infer something about the realization of the random variable a. Thus their expected utility is *not* given by relationship 18. Since, however, the cut point voter $\tilde{x}_R$ has to lie in between $\bar{x}^D$ and $\bar{x}^R$, given condition 9, this consideration does not affect the proof of Proposition 1. We can then establish the following result.

PROPOSITION 1. *The unique cut point* $\tilde{x}_R$ *is given by* $\tilde{x}_R = (\bar{x}^R + k)/(1 + k)$.

Proof. Given our definition of equilibrium, a necessary and sufficient condition to identify the bliss point of the pivotal voter is the following:

$$\frac{\partial EU^{Ri}}{\partial \tilde{x}_R} \gtreqless 0 \quad \text{if } x^i \lesseqgtr \tilde{x}_R.$$

Thus

$$\frac{\partial EU^{Ri}}{\partial \tilde{x}_R} = \int_{a^{\min}}^{a^{\max}} (\bar{x}^R + k(1 + a - \tilde{x}_R) - x^i)\, kf(a)da. \tag{19}$$

Remembering that

$$\int_{a^{\min}}^{a^{\max}} af(a) = 0$$

relationship 19 implies that

$$\frac{\partial EU^{Ri}}{\partial \tilde{x}_R} \gtreqless \text{ if } \tilde{x}_R \lesseqgtr \frac{\bar{x}^R + k}{1 + k}.$$

Therefore the unique cut point is $\tilde{x}_R = (\bar{x}^R + k)/(1 + k)$. QED

Thus, using relationships 14 and 17 and the result of Proposition 1, we obtain

$$E(\pi^R_{t+1}) \equiv \pi^e_{t+1} = \tilde{x}_R = \frac{\bar{x}^R + k}{1 + k} \tag{20}$$

and

$$\pi^R_{t+1} = \frac{\bar{x}^R + k}{1 + k} + ka$$

Consider the case in which a Democratic president is in office in period t and define the cut point $\tilde{x}_D$ by $V^{DD} = 1 + a - \tilde{x}_D$. By repeating the same procedure described before we obtain[8]

$$E(\pi^R_{t+1}) \equiv \pi^e_{t+1} = \tilde{x}_D = \frac{\bar{x}^D}{1 + q} \tag{21}$$

and

$$\pi^D_{t+1} = \frac{\bar{x}^D}{1 + q} + qa.$$

We now turn to the presidential election of period t. (Recall that we are assuming that in the first period the president is unconstrained by Congress.) The voters have all the information necessary to know which policy would be followed by whoever is elected president in period t. In addition voters can compute the expected policies for period $t + 1$ by using relationships 20 and 21. Thus in period t voter i, with preferences u^i, votes for the Democratic candidate if and only if the following holds:

$$E[u^i(\bar{x}^D) + \beta u^i(\pi^D_{t+1})] > E[u^i(\bar{x}^R) + \beta u^i(\pi^R_{t+1})]. \tag{22}$$

Relationship 22 underscores that voter i is better off if a Democratic president is elected in period t taking account of the expected effects of the midterm congressional elections. Thus, the voters for the Democratic president have bliss points satisfying the following inequality:

$$x^i > A/B$$

where

$$\begin{aligned} A = {} & \beta\{[E(\pi^D_{t+1})]^2 - [E(\pi^R_{t+1})]^2 \\ & + \text{var}(a)(q^2 - k^2)\} \\ & + \bar{x}^{D2} - \bar{x}^{R2} \\ B = {} & 2\{(\bar{x}^D - \bar{x}^R) \\ & + \beta\,[E(\pi^D_{t+1}) - E(\pi^R_{t+1})]\} \end{aligned} \tag{23}$$

In relationship 23 $E(\pi^D_{t+1})$ and $E(\pi^R_{t+1})$

are defined by relationships 20 and 21, and var(a) is the variance of a.

Let us define $\hat{x}$ as the bliss point of the voter who is indifferent between voting Republican or Democratic for president in period t; thus $\hat{x}$ is such that relationship 23 holds as an equality. In Appendix A, the following result is shown:

PROPOSITION 2. *If* $q = k$, *then the following inequalities hold:* $\bar{x}^R < \tilde{x}_R < \hat{x} < \tilde{x}_D < \bar{x}^D$.

This result is consistent with the empirical observation that the party of the president always loses votes in midterm elections. In fact Proposition 2 shows that there are always voters who switch from the party of the president to the opposing party in order to counterbalance the president's policy in the second half of the term. If $q \neq k$, that is, if one of the two potential presidents is more responsive than the other to the composition of Congress, the inequalities of Proposition 2 still hold if var(a) is not too big (see Appendix A).

Proposition 2 also establishes a relationship between the economic outcome in the first half of each administration and the midterm electoral results. The basic point is that other things being equal, the more polarized the two parties bliss points are, that is, the more distant $\bar{x}^D$ and $\bar{x}^R$ are, the bigger the deviation of output growth from its natural level and the larger the fraction of voters switching from one party to the other in the midterm elections. In fact, consider an increase in the distance between $\bar{x}^D$ and $\bar{x}^R$ such that the cut point of the presidential election ($\hat{x}$) remains unchanged. Then the probability of electing a Democratic or Republican president remains unchanged. In this situation it follows from relationship 13 that the deviation of output from its natural level ($\bar{y}$) increases; that is, one observes a bigger expansion or recession in the first half of a Democratic or Republican administration, respectively. For instance, consider the case in which a Republican wins the presidential election. Since $\bar{x}^R$ has decreased, so does $\tilde{x}_R$ (from relationship 20). This implies that a larger fraction of voters switches from the Republican to the Democratic party in midterm elections.

The same qualitative implications for the midterm electoral cycle holds if Congress is elected every period, that is, in presidential election years as well as in "off" years. In this case the voters can achieve some counterbalancing effect in the first period, too. The complete solution of this case is presented more extensively in Alesina and Rosenthal 1988. Here we simply sketch the solution of the first-period problem, in order to show that the midterm electoral cycle survives this generalization.

When both elections (congressional and presidential) occur contemporaneously in the first period, two cut points are relevant: $\hat{x}$, the cut point for the presidential election and $\tilde{x}$, the cut point for the congressional election. The expected utility of voter i depends on both cut points. The cut point $\hat{x}$ determines $P(\hat{x})$, that is, the probability of electing a Democrat president. Let us define as interior an equilibrium in which $\hat{x}$ and $\tilde{x}$ are such that $0 < P(\cdot) < 1$ and the following condition

$$\left.\frac{\partial EU^i(\hat{x},\tilde{x})}{\partial \tilde{x}}\right|_{x^i = \tilde{x}} = 0$$

is satisfied.

For our present purposes it is sufficient to establish the following result.

PROPOSITION 3. *If an interior equilibrium in the first period exists, then* $\tilde{x}_R < \tilde{x} < \tilde{x}_D$.

Proof. Suppose not. For given $\hat{x}$, $\tilde{x}$ satisfies the condition

$$\frac{\partial EU^i(\hat{x},\tilde{x})}{\partial \tilde{x}} = P \left.\frac{\partial EU^{Di}(\hat{x},\tilde{x})}{\partial \tilde{x}}\right|_{x^i = \tilde{x}}$$

$$+ (1 - P)\left.\frac{\partial EU^{Ri}(\hat{x},\tilde{x})}{\partial \hat{x}^i}\right|_{x^i = \tilde{x}}$$

$$= 0, \qquad (24)$$

where P = probability of electing a Democratic president, which is a constant for a given $\hat{x}$; $EU^{Di}(\bullet)$ = expected utility of voter i if a Democrat is elected; and $EU^{Ri}(\bullet)$ = expected utility if a Republican is elected. It is easy to show that if $\tilde{x} \leq \tilde{x}_R$ or $\tilde{x} \geq \tilde{x}_D$,

$$\frac{\partial EU^{Di}(\hat{x},\tilde{x})}{\partial \tilde{x}} \text{ and } \frac{\partial EU^{Ri}(\hat{x},\tilde{x})}{\partial \tilde{x}}$$

either have the same sign or one is zero and the other nonzero. Therefore relationship 24 cannot be satisfied. QED

The intuition of this result is straightforward. If, say, $\tilde{x} < \tilde{x}_R$, the voters with bliss points x^i such that $\tilde{x} < x^i < \tilde{x}_R$ act as follows: in the first period, under uncertainty about the president's identity, they vote Democratic for Congress, implying that they would want to counterbalance a Republican president; in the second period when they know that a Republican president is in office, they switch to voting Republican for Congress, reducing the counterbalancing effect. This behavior cannot be rational; thus $\tilde{x} < \tilde{x}_R$ cannot be an equilibrium. An analogous argument holds for $\tilde{x} > \tilde{x}_D$.

Alesina and Rosenthal (1988) show that under mild sufficient conditions an "interior equilibrium" in the first period exists and that, in general, $\hat{x} \neq \tilde{x}$; that is, there is ticket splitting in the first period. An interior equilibrium is obtained if there is enough uncertainty (i.e., if $[a^{\max} - a^{\min}]$ is sufficiently high). An explicit solution for $\hat{x}$ and $\tilde{x}$ can be obtained if $q = k$ and the distribution of a is uniform. Simulation results show, however, that these simplifying assumptions are not necessary for existence.

Aggregate Results for Congressional Elections 1950–84

Our empirical analysis focuses on House and Senate elections from 1950 to 1984. Undoubtedly, some previous investigators had shied away from Senate data because only two-thirds of the states hold Senate elections in a given election year. In fact, results for the Senate are qualitatively similar to those for the House. Testing for "class" effects by including dummy variables generally proved negative. In order to gain degrees of freedom, observations from the interwar period could be used. In this case, however, one has to make more and more heroic assumptions about the stability of the politicoeconomic environment and thus about the stability of coefficients. In addition, if one excludes the observations affected by the two world wars and by the Great Depression, very few useful observations can be obtained for years prior to 1950 in this century.[9]

Incumbency Models

We begin our data analysis by considering the simplified model of the political process developed in the preceding section. In the model we assumed a perfectly stable environment except for the stochastic variation in the distribution of voters' ideal points, with the result that no variation is predicted in the presidential vote other than that induced by the stochastic process. Thus, this simple model does not suggest any systematic variation in either the presidential vote or the congressional vote in "on" years.[10] In "off" years, in contrast, the model predicts an increase in the vote for the Republicans when the Democrats hold the White House and vice-versa. Given the small number of data points, we assume the two balancing effects to be equal. Thus, we have the following specification: $V_{R,t} = \beta_0 + \beta_1 M_t + \epsilon_t$ where $V_{R,t} \equiv$ Republican percentage of two-party vote in year t. $M_t \equiv 1$ if incumbent president is Democratic and t is "off" year, -1 if incumbent president is Republican and t is "off" year, and 0 otherwise; and ϵ_t satisfies the standard ordinary least squares assumption (consistent with our model).

As to the coefficients, we hypothesize $\beta_0, \beta_1 > 0$. As shown in column 1 of Table 2, the simple model is supported by the data, with conventional significance levels at .05 or better. The model predicts that the two-party vote will split near its long-term average in presidential years and favor the "outs" by 3.2% in the Senate and 1.8% in the House in midterm elections.

Our simple model captures part of the aggregate fluctuations but leaves much unexplained variance. An alternative model emphasizes a systematic incumbency effect pertaining to both "on" and

Table 2. Incumbency Regressions

Variable	(1)	(2)	(3)	(4)	(5)
			House of Representatives		
Constant	46.47	46.51	46.41	46.41	46.43
	(.57)	(.58)	(.57)	(.48)	(.47)
M-midterm[a]	1.82	1.21	1.81	-.97	—
	(.81)	(1.15)	(.80)	(1.20)	
I-incumbent[b]	—	.61	—	2.79	2.11
		(.82)		(1.00)	(.55)
DEPR[c]	—	—	-1.15	-3.93	-3.26
			(1.08)	(1.34)	(1.04)
Standard error of estimate	2.42	2.42	2.41	2.00	1.97
R^2	.24	.27	.29	.55	.53
DW	1.13	1.24	1.14	2.03	1.66
			Senate		
Constant	47.67	47.66	47.50	47.50	47.50
	(.70)	(.73)	(.60)	(.44)	(.42)
M-midterm	3.21	3.37	3.19	-.24	—
	(.99)	(1.45)	(.84)	(1.11)	
I-incumbent	—	-.16	—	3.43	3.27
		(1.03)		(.92)	(.50)
DEPR	—	—	-3.06	-6.49	-6.33
			(1.13)	(1.24)	(.94)
Standard error of estimate	2.97	3.07	2.52	1.85	1.79
R^2	.40	.40	.59	.80	.80
DW	1.84	1.83	1.88	2.76	2.70

Note: The dependent variable is the Republican percentage of the two-party vote. Numbers in parentheses are standard errors.

[a] $M = 1$ for Democratic incumbent "off" year, -1 for Republican incumbent "off" year, 0 otherwise.

[b] $I = 1$ for Democratic incumbent, -1 for Republican incumbent.

[c] $DEPR = 1$ for Democratic incumbent candidate "on" year, -1 for Republican incumbent candidate "on" year, 0 otherwise.

"off" years. We thus define $I_t \equiv 1$ if incumbent president is Democrat and -1 if incumbent president is Republican.

This incumbency variable was included in the specifications of Kramer (1971) and Fair (1978, 1982, n.d.). As seen in column 2 of Table 2, the standard incumbency variable, by itself, does not furnish explanatory power beyond that available in the midterm model.

In addition to the standard incumbency variable, Fair has considered an additional incumbency measure. He defines $DEPR_t \equiv 1$ if Democratic incumbent president runs for reelection in an "on" year, -1 if Republican incumbent president runs for reelection in an "on" year, and 0 otherwise.

Following Fair (n.d.), we treat Gerald Ford as a nonincumbent in 1976. As shown in column 3 of Table 2, inclusion of *DEPR* by itself leaves our estimate of the midterm effect virtually unaffected, although the fit for the Senate is improved substantially. However, when *M*, *I*, and *DEPR* are all included in the model, there is a strong increase in fit and the midterm effect becomes unimportant (column 4). Indeed, of the various incumbency models, the best model, in terms of the ex post estimated standard error, includes only *I* and *DEPR*, shown in column 5 of Table 2.

With respect to the results in column 5, note that a midterm effect is still maintained. It is given by the coefficient on *I* and is 2.1% in the House and 3.3% in the Senate, quite close to the original estimates in column 1. On the other hand, column 5 contradicts the simple model's hypothesis of only random variation in presidential years. When the incumbent runs for reelection, the congressional party reaps benefits (equal to *I* coefficient minus *DEPR* coefficient). This incumbency bonus is 1.1% in the House and 3.2% in the Senate.

The *DEPR* results need, however, to be viewed with caution. If we let $I^* = I - M$, regressing on I^* and *M* is equivalent to regressing on *I* and *M*. In turn, *DEPR* differs from I^* only in its treatment of four observations, one of which involves the uncertain coding of Ford. As Fair (n.d.) also noted for the presidency, changing the Ford coding decision substantially alters the results. The column 4, R-squared values drop from .55 to .40 for the House and from .80 to .68 for the Senate. The *DEPR* coefficient is not significant at the .05 level in either colummn 4 or 5 for the House. More fundamentally, *DEPR* is not entirely a variable predetermined before the electoral period. Truman in 1952 and Johnson in 1968 were both eligible for reelection. In both cases their decision not to pursue another term may have reflected their and their party's temporary unpopularity as a result of prolonged military conflicts in Asia. Consequently, *DEPR* might well be regarded as an endogenous variable, and columns 3–5 are subject to simultaneous equations bias. Of course, it might well make sense, in the spirit of Fair's research, to include *DEPR* in a short-term forecasting model (Truman announced his withdrawal on 12 March 1952, Johnson on 30 March 1968).

What is truly predetermined is whether the chief executive is a lame duck. Only two observations in our sample—1960 and 1976—are in this lame duck situation. If *DEPR* is recoded to differentiate lame ducks from others, there is a further drop in R-squared, to .36 for column 4 for the House and to .53 for the Senate, while the lesser improvement afforded over column 2 now depends solely on two observations, 1960 and 1976. All in all, there is reason to regard the *DEPR* effects with skepticism. Nonetheless, the results suggest that presidential election years contain important lagged incumbency effects that are not captured in our simple model, which treats each four-year term as an independent event.

Economic Influences Models

Another set of alternatives to our simple model is posed by various models of economic influences on voting behavior. The essence of the Kramer and Fair models is that the incumbent does well in good times. Growth in real per capita GNP, real per capita income, unemployment, and the inflation rate have been used as indicators of performance. Fair and Kramer find the strongest effects for the first two indicators, and Bloom and Price (1975) consider only the second. Consequently, we restrict ourselves to the first two measures, denoted by g and i respectively.[11] Because we have few observations—and in order to avoid collinearity—we use these measures one at a time. Kramer (1971), Fair (1978), and Bloom and Price (1975) all use annual data for the year of the election. Fair (n.d.) concludes in favor of even shorter voter horizons and used data for the second and third quarters preceding the election. We consider both variants.

There are several possible models. (See also Arcelus and Meltzer 1975 and Tufte 1975.) One that Kramer noted for historical reasons but did not investigate is that the Republicans do well in good times. From the viewpoint of our theoretical model, this traditional lore is in fact plausible, since "good times," defined in terms of high GNP growth, but with inflation, should tend to occur in the first half of a Democratic presidency. The regressor for this model, when GNP is the indicator, would simply be g. In the Kramer and Fair models, good times favor the incumbent, so the regressor is $-gI$. Bloom and Price adapt the Kramer model to allow for a "switching" regression. In recessions ($g < 0$), voters react to performance more than in expansions. The Bloom and Price model can be written: $V_{R,t} = \beta_0 + \beta_1[-g_t^+ g_t I_t] + \beta_2[-(1-g_t^+)g_t I_t] + \epsilon_t$ where $g_t^+ = 1$ if $g_t > 0$ and 0 otherwise.

The hypothesis of the model is that $\beta_2 > \beta_1 \geq 0$.

In contrast to these models, our simple approach suggests that if economic factors relate to voting at all it will only be a spurious correlation produced by the expectation uncertainty of economic agents and the moderating behavior of voters. The relevant economic variable should be deviation from trend. We consider $(g - \alpha)^2 I$, where α is a target level for the economy. As in the other models, the coefficient on this variable should be positive. We either set α equal to the average growth rate of the economy over the time period of our data set or treat α as a parameter to be estimated by nonlinear least squares. Another interpretation of this model is that voters are retrospective but more sophisticated than the "naive" voters of Kramer, Fair, and Bloom and Price. Sophisticated voters do not view too-high growth in the short run as desirable. Such growth may be seen as potentially inflationary, or, more generally, voters may prefer a stable growth path rather than fluctuations of growth. This view of the model is congruent with the approach of Chappell (1983) and Chappell and Keech (1985). The model with squared deviation also is congruent with Lepper's (1974) finding that incumbents lose votes when either unemployment or inflation is high. Note that not all observations discriminate among the models. Our squared deviation model will look much like a Republican prosperity model as long as prosperity coincides with Democrats in office and recessions coincide with Republican administrations. Similarly, squared deviations parallel Bloom and Price in calling for recessions to be severely punished. Squared deviations and Kramer and Fair basically agree for $g < \alpha$. Consequently, with only 18 observations, it is difficult to discriminate between the models.

We begin our discussion of "economic" models by considering simple GNP

Table 3. Simple Economic Influences Regressions

	House of Representatives				
				Squared Deviation	
Variable	Prosperity	Kramer-Fair	Bloom-Price	Fixed	Estimated
Constant	45.45* (.85)	46.18* (.67)	46.60* (.69)	46.59* (.57)	46.83* (.81)
g^a	.31 (.20)	—	—	—	—
$-gI^b$	—	-.15 (.16)	—	—	—
$-g^+gI^c$	—	—	-.16 (.15)	—	—
$-(1-g^+)gI$	—	—	1.27 (.97)	—	—
$(g-\alpha)^2 I$	—	—	—	.10* (.04)	.09 (.07)
α^d	—	—	—	2.96 (fixed)	3.77* (1.88)
Standard error of estimate	2.58	2.70	2.59	2.40	2.23
R^2	.14	.05	.18	.25**	.27**
DW	1.46	2.05	1.51	1.45	—
2lnL	—	—	—	—	-80.01
	Senate				
Constant	45.31* (.96)	46.18* (.67)	48.17* (.92)	46.59* (.57)	48.54* (.69)
g	.74* (.22)	—	—	—	—
$-gI$	—	-.06 (.22)	—	—	—
$-g^+gI$	—	—	-.08 (.20)	—	—
$-(1-g^+)gI$	—	—	2.58 (1.25)	—	—
$(g-\alpha)^2 I$	—	—	—	.13* (.06)	.06 (.08)
α	—	—	—	2.96 (fixed)	7.00 (4.33)
Standard error of estimate	2.94	3.81	3.45	3.39	2.90
R^2	.41**	.01	.24	.21	.35**
DW	2.28	2.68	2.33	2.53	—
2lnL	—	—	—	—	-89.42

Note: Numbers in parentheses are standard errors.

[a] See Appendix B.

[b] See Table 2.

[c] The variable $g^+ = 1$ if $g > 0$, $= 0$ otherwise.

[d] Target growth rate parameter.

*Coefficient significantly differs from 0 at .05 level or better (one-tailed).

**F-test for regression significant at .05 level or better.

models, using growth rates from the year of election, as shown in Table 3. An immediate observation is that none of the models substantially outperforms any of the incumbency models, including the very simple midterm effects model. The Kramer and Fair model does particularly poorly, and the Bloom and Price model also does not do very well. None of these "naive" retrospective regressions passes the standard .05 level. In terms of the standard error of the estimate (SE) both are dominated by the prosperity and squared deviation models. The negative results for the Kramer and Fair and Bloom and Price models are important when viewed in the context of these authors' results. In Kramer's original paper a simple economic model based on real income was highly significant. As he added variables to this initial regression, the economic growth variable remained the driving variable. In the case of Bloom and Price the simple model was the only one they presented. Our results indicate that these earlier retrospective voting models are not robust to alterations in the time period and data series.

The prosperity model does poorly for the House but quite well for the Senate. The squared deviation model with fixed α is more stable. The squared deviation variable is significant for both chambers. The slope estimates for the House and Senate are quite similar. Estimating the "target" parameter α improves the fit slightly in the House and substantially in the Senate. The data, however, do not permit obtaining precise estimates of both α and the coefficient on the squared deviation. We thus cannot reject the null hypothesis that $\alpha = 2.96$, the average GNP growth rate in the 1950–84 period. On the whole, the evidence in Tables 2 and 3 is at least as supportive of our balancing model as of retrospective voting models. To sum up,

1. Simple retrospective voting models do not outperform the model of Republicans' doing well under prosperity.
2. Simple retrospective voting models do not outperform the model of incumbents' being punished for deviations from a target level of growth.
3. None of the models based on GNP growth rates for the year of the election fits the data substantially better than the simple midterm effect model (column 1 of Table 2).

One important claim of proponents of retrospective voting models is that the horizon employed by the voters is very short. However, our theoretical model would suggest that two years—rather than one year—prior to the election would be relevant. The reason is that the spurious correlation between voters' moderating behavior and the partisan business cycle is likely to be improved by the averaging implied in including data for both years prior to an election. Consequently, we reestimated all the models shown in Table 3 by also including all relevant variables lagged one year. Results are shown in Table 4. To save space, we group the g variables for the various models in a block of lines. The variables and their orders are as shown in Table 3.

Our hypothesis concerning the squared deviation models is confirmed. For fixed α, both lagged coefficients are significant.[12] Consistent with the averaging we expected, we cannot reject the hypothesis that the coefficients on the squared deviation variables are equal. When α is estimated, there is a significant improvement in the log likelihood for both houses. At the same time, the retrospective voting models are "improved" for the Senate (but not the House). The F-test for Kramer and Fair is significant. The driving variable, with the wrong sign, is lagged GNP. Bloom and Price's lagged recession variable is also significant. Their model shows at least as large an effect from negative growth one year prior to the election as in

Table 4. Economic Influences, Lagged Regressions

	House				
				Squared Deviation	
Variable	Prosperity	Kramer-Fair	Bloom-Price	Fixed	Estimated
Constant	45.45* (1.22)	46.30* (.67)	47.04* (.73)	46.41* (.54)	46.83* (.81)
g variables[a]	.32 (.19)	–.05 (.19)	.05 (.21)	.08* (.04)	.05 (.09)
	—	—	1.27 (.97)	—	—
g_{-1} variables[b]	.27 (.24)	–.16 (.18)	–.17 (.20)	.08* (.04)	.11 (.18)
	—	—	3.58 (2.33)	—	—
α[a]	—	—	—	2.96 (fixed)	4.67* (2.31)
Standard error of estimate	2.56	2.72	2.54	2.21	1.82
R	.20	.10	.32	.41	.52**
DW	1.60	1.86	1.82	1.90	—
2lnL	—	—	—	—	-72.56
	Senate				
Constant	43.64* (1.32)	47.77* (.94)	48.81* (.90)	47.48* (.72)	47.93* (.66)
g variables	.75* (.21)	.24 (.24)	.37 (.26)	.10* (.06)	.10 (.09)
	—	—	2.11* (1.18)	—	—
g_{-1} variables	.45 (.26)	–.49 (.23)	–.50 (.24)	.12* (.05)	.13 (.22)
	—	—	4.74* (2.84)	—	—
α	—	—	—	2.96 (fixed)	3.93* (1.46)
Standard error of estimate	2.78	3.44	3.45	2.98	2.58
R^2	.50	.24**	.47	.43**	.49**
DW	2.25	2.30	2.41	2.66	—
2lnL	—	—	—	—	-85.25

Note: Numbers in parentheses are standard errors.

[a] See Table 3 for definitions.

[b] Table 3 variables lagged one year.

*Coefficient significantly differs from 0 at .05 level or better (one-tailed).

**F-test or likelihood ratio test for including lagged terms in regression significant at .05 level or better.

the year of the election. Although the coefficients are not estimated precisely, the House runs are similar—retrospective voting models show larger effects from the lagged data. So we have

4. The data do not support the claims of voter myopia found in the retrospective voting literature.
5. The squared deviation models are significantly improved by including GNP data for the year preceding the election year.

To address the emphasis found in Kramer and Bloom and Price, we redid all the regressions in Tables 3 and 4 using **i** in place of **g**. We summarize the result:

6. The income measure is not a good candidate for an explanatory variable in simple economic models of aggregate congressional voting. The fits are generally worse than those for the GNP variable. When the fits are better, coefficient values are not as hypothesized by the naive retrospective voting models.

In his series of studies on presidential voting, Fair has also focused on real GNP as the main predictor. Fair (n.d.) claims (at least for presidential elections) that it is mainly *per capita* growth in the second and third quarters of an election year that matters. Consequently, we also estimated the models of Table 3 and the no-lags columns of Table 5 with g_t redefined to be the annualized per capita growth in the second and third quarters of year t.[13] Again the results are negative. The prosperity and Kramer and Fair models are always worse for the two-period growth rate than for the growth rate for all of the election year. The squared deviation model is better for the House, with incumbency and trend, and worse elsewhere. The Bloom and Price model does better in the House run (R^2 = .29 vs. .18) but worse in the Senate (.09 vs. .21).

Our comparisons of the various models of economic influences on voting behavior have been limited to simple linear regressions including an economic variable and its lag or, in the case of Bloom and Price, a piecewise linear regression. The literature, however, includes variables that measure noneconomic influences. Specifically, both Kramer and Fair included I and t (time trend) as additional regressors. The results, presented in Table 5, of comparing the Kramer and Fair model and the squared deviation model when I and t are included are quite instructive.

All the results in Table 5 must be viewed with caution, given the number of coefficients relative to degrees of freedom. Indeed, only two of the eight F-tests (testing the null hypothesis that all coefficients, save the constant, are zero) for the overall regression are significant at the .05 level. Neither of the two models dominates. Without the lagged variable, the Kramer and Fair model always has a better fit than the squared deviation. Indeed, the lagged variable does not noticeably improve the fit for Kramer and Fair, in line with the previous retrospective voting lore. However, the squared deviation model, with the lag, is nearly as good as the Kramer and Fair model for the House and substantially better for the Senate. This gives us

7. In a specification that includes an incumbency variable and a time trend there is no clear case for a Kramer and Fair retrospective voting model, based on short memory, versus a squared deviation model, in which lags are important.

What is most interesting about Table 5, however, is the pattern of the coefficients. Introducing incumbency into the Kramer and Fair model makes a dramatic difference in fit. (Compare Table 5 with Tables 3 and 4.) Incumbency costs the

Table 5. Economic Influences Regressions: GNP, Incumbency, and Trend

Variable	Kramer-Fair		Squared Deviation	
	No Lags	Lags	No Lags	Lags
House				
Constant	49.31* (1.40)	49.38* (1.43)	46.67* (1.25)	46.69* (1.39)
g variable	.38 (.22)	.40 (.23)	.05 (.07)	.09 (.07)
g_{-1} variable	—	.16 (.23)	—	.08 (.05)
I	2.21** (.85)	2.84** (1.26)	.66 (.86)	-.19 (1.02)
t^a	-.23 (.12)	-.23 (.12)	-.12 (.12)	-.03 (.13)
α	—	—	2.96 (fixed)	2.96 (fixed)
Standard error of estimate	2.26	2.30	2.45	2.29
R^2	.42***	.44	.32	.41
DW	1.51	1.47	1.69	1.91
Senate				
Constant	50.56* (1.98)	50.49* (2.04)	47.56* (1.83)	45.14* (1.72)
g variable	.68* (.32)	.66* (.33)	.10 (.10)	.20* (.09)
g_{-1} variable	—	-.16 (.32)	—	.19* (.07)
I	3.50** (1.20)	2.85 (1.26)	.63 (1.26)	-1.47 (1.26)
t	-.19 (.16)	-.19 (.17)	.02 (.18)	.24 (.16)
α	—	—	2.96 (fixed)	2.96 (fixed)
Standard error of estimate	3.20	3.29	3.59	2.92
R^2	.39	.40	.23	.53***
DW	2.33	2.28	2.50	2.81

Note: Numbers in parentheses are standard errors.

[a] Time in years, 1950 = 1. See Tables 3 and 4 for other variable definitions.

*Coefficient significantly differs from 0 at .05 level or better (one-tailed).

**Coefficient significantly differs from 0 and .05 level or better (two-tailed).

***F-test or likelihood ratio test for regression significant at .05 level or better.

"ins" about 2.2% of the vote in the House and 3.5% in the Senate. These figures are quite close to our original estimate of the midterm effect. While GNP growth now has the correct sign, it clearly has a subsidiary role to incumbency and has a significant coefficient only in the House runs. For GNP growth to have an effect roughly equal to that of incumbency, growth would either have to fall from its average level of around 3% to about −2% (a major recession level attained by only 1982 in the data) or accelerate to 8% (a boom level that occurred only before the 1950 election).

In contrast to the Kramer and Fair model, the squared deviation model estimates never show a significant incumbency effect. The estimates for incumbency are never as much as one-third those in the corresponding Kramer and Fair model and, moreover, are negative in the lagged model. Adding incumbency helps the overall fit of the original simple model only slightly. The GNP coefficient estimates retain the signs found in Tables 3 and 4 but are significant only in the lagged model for the Senate. What has happened is that the coefficients are not precisely estimated as a result of the predicted collinearity between I and $(g - \alpha)^2 I$.

When we run a full Fair (n.d.) model (minus the weak, nonsignificant inflation variable) by including *DEPR*, we obtain results similar to the simple regression shown in Table 2. Just as the M coefficient was near zero when both I and *DEPR* were included, the squared deviation coefficients are near zero when *DEPR* is added to the model of Table 5. While the naive retrospective g variable is slightly more successful in this context (again a lagged version is more favorable to squared deviations), the critical variables in Fair's specifications are noneconomic. In both chambers the change in GNP growth from the average would have to be over 10% for the estimated impact to equal that of I. Such a change is beyond all the sample values of g. Similarly, according to the *DEPR* model, a party will gain more from getting its incumbent to stand for another term than it could ever possibly achieve by successful management of the economy. Although the *DEPR* model puts a twist on presidential factors not captured in our model, the basic empirical results square with the thrust of our model—elections are not driven by economic performance but by forces internal to the political process.

Our simple theory, of course, also predicts that the squared deviation model will not improve on the simple midterm effect variable M. This observation brings us directly to our empirical punch line. Neither GNP variable improves on our simple midterm effect model. We show this in Table 6, where the I dummy variable in Table 5 has simply been replaced by M. (The I variable still appears in the g variables.) Substituting M for I in a classical Kramer model is something of a draw—the House results are worse and the Senate results are better. In both cases, however, GNP growth has a negligible effect. The only significant coefficient appears in the lagged Senate model, but there the lagged coefficient has the wrong sign while the sum of the two coefficients is roughly zero. A similar story holds for squared deviations. Here the R-squared is better than with I for both Senate and House. No coefficients are significant in the House run, whereas they are significant when M and squared deviations are run separately. The lack of significance in Table 6 reflects the anticipated collinearity between the two variables. One lagged coefficient is significant in the Senate, but—no surprise at this point—the unlagged coefficient has the wrong sign. Thus,

8. Simple models of economic influences add little, if anything, to the basic midterm effects model.

Table 6. Economic Influences Regressions: GNP, Midterm, and Trend

	House			
	Kramer-Fair		Squared Deviation	
Variable	No Lags	Lags	No Lags	Lags
Constant	48.24*	48.43*	47.87*	47.08*
	(1.37)	(1.44)	(1.26)	(1.30)
g variable[a]	.07	.13	.02	.02
	(.16)	(.19)	(.08)	(.08)
g_{-1} variable	—	−.10	—	.07
		(.18)		(.04)
M	1.84*	1.63	1.46	1.20
	(.88)	(1.26)	(1.45)	(1.39)
t	−.18	−.19	−.15	−.08
	(.12)	(.13)	(.12)	(.13)
α	—	—	2.96	2.96
			(fixed)	(fixed)
Standard error of estimate	2.40	2.46	2.41	2.30
R^2	.35	.36	.34	.44
DW	1.22	1.22	1.34	1.71
	Senate			
Constant	49.03*	49.62*	48.31*	46.77*
	(1.72)	(1.68)	(1.64)	(1.47)
g variable	.25	.42*	−.06	−.05
	(.21)	(.23)	(.11)	(.09)
g_{-1} variable	—	−.32	—	.13*
		(.21)		(.05)
M	3.70*	3.05*	4.07*	3.57*
	(1.10)	(1.26)	(1.89)	(1.57)
t	−.11	−.15	−.08	.06
	(.15)	(.15)	(.16)	(.14)
α	—	—	2.96	2.96
			(fixed)	(fixed)
Standard error of estimate	3.02	2.88	3.13	2.60
R^2	.45***	.54***	.41***	.63***
DW	1.66	1.44	1.76	1.91

Note: Numbers in parentheses are standard errors.

[a] See Tables 2, 3, 4, and 5 for variable definitions.

*Coefficient significantly differs from 0 at .05 level or better (one-tailed).

**Coefficient significantly differs from 0 at .05 level or better (two-tailed).

***F-test or likelihood ratio test for regression significant at .05 level or better.

Conclusion

We have presented a simple but internally consistent macromodel of an economic and political system. The model is obviously very crude, and further theoretical work will need both to bring in dynamics that would enrich the two-period focus of the current model and to relax simplifying technical assumptions. The model we have presented does, however, capture some critical features of some modern political systems.

The economic side of the model accounts for the notion that economic agents cannot be routinely fooled by the government. With rational expectations, anticipated efforts at manipulating the economy should have no effect on the real level of output. On the other hand, agents enter into contracts that are sufficiently long-term that they can only attempt more or less successfully to hedge against the uncertainty generated by politics.

In addition to the assumption of voters' rationality, the political side of the model has three key features: (1) a two-party system in which politicians are polarized, (2) policy that reflects the influence of both the executive and the legislature, and (3) an institutional structure with legislative elections held while the executive remains in power. The United States and France are the two reasonably good fits to our stylized environment.

The upshot of our stylized "political economy" is that political polarization and uncertainty generate economic fluctuations and that voters use the legislative elections to attenuate the policy swings engendered by polarization. Within the context of our model these concurrent economic and political events generate a purely spurious correlation between current economic conditions and voting behavior in the legislative elections.

In our empirical section we provide evidence for the spurious correlation. The midterm effect portrayed by our model was strongly supported by the data, while various "economic" models of voting behavior failed to generate much additional explanatory power.

Our empirical results help to reconcile the findings from survey data that individual voting behavior is not responsive to changes in individual economic outcomes (Kiewiet 1981; Kinder and Kiewiet 1979) and the supposed regularity that aggregate voting behavior responds to aggregate economic conditions. While Kramer (1983) provided an elegant methodological reconciliation based on the observation that the survey questions failed to differentiate overall individual changes in economic fortunes from the portion of the change the individual attributed to government, our findings question the existence of the regularity for the postwar period, at least so far as congressional elections are concerned.

The near-zero correlations we have obtained between standard "retrospective" regressors and *congressional* outcomes contrasts with the very high R-squared values Fair (n.d.) has obtained for *presidential* elections. Markus (1988), using survey data, also finds strong support for retrospective voting using survey data for the 1956–84 period. (See also Chubb 1988 on state legislative elections.) It is thus quite possible that both retrospective and moderating influences are present. In voting for the presidency, voters use the past to evaluate the current presidential party but—having selected the chief executive—invariably choose moderation in the off-year elections.

Our model has also not considered many other important influences on voting behavior. Just before this paper went to the editor, an important paper by Erikson (1988) appeared. After a careful statistical analysis of alternative hypotheses about the midterm effect, he concludes, "Standard interpretations in terms of withdrawn coattails (or regression to the mean), surge and decline, [and] a negative referendum verdict on presidential performance are all incompatible with the empirical evidence" (p. 1028). Erikson does find support for a "presidential penalty" model similar to ours, in which, "to create a balance of power between the two parties, . . . voters would be motivated to oppose the presidential party at midterm" (p. 1014).

What we have added to Erikson's framework (and that of Fiorina [1988]) is an explicit consideration of the coordination problem facing the voters and a treatment of how uncertainty interacts with the coordination problem. An important implication of uncertainty for our model is that hedging causes the presidential year cut point for the congressional vote to be intermediate between those of the two midterm cut points. Unless one party always won the presidency, we would thus predict more variance in the midterm series than in the presidential year series. The data, as previously noted by Jacobson and Kernell (1983), are strongly in accord with this prediction.

In addition to the midterm effect, our model predicts split ticket voting. Obviously, there are other explanations for split tickets, most notably, each voter's incentive to feather the constituency's congressional nest by continuing to reelect the current incumbent. Although Fiorina (1988) reports substantial split ticket voting in "on" years in open seats, the incumbency incentives are probably the major factor in limiting the midterm effect to only about 2.5% of the electorate. That the midterm effect exists at all responds to the fact that open seats arise and to the possibility that many individuals (e.g., professors who participate in national labor markets, retirees) are more responsive to the direction of national economy policy than to increments to local pork.

Appendix A: Proof of Proposition 2

The inequalities $\bar{x}^R < \tilde{x}_R < \tilde{x}_D < \bar{x}^D$ can be easily established by using relationships 9, 16, 20 and 21. The inequalities $\tilde{x}_R < \hat{x} < \tilde{x}_D$ can be established as follows. If $q = k$, relationship 23 can be rewritten as an equality as follows:

$$\hat{x} = \frac{\beta(\tilde{x}_D^2 - \tilde{x}_R^2) + \bar{x}^{D2} - \bar{x}^{R2}}{2[(\bar{x}^D - \bar{x}^R) + \beta(\tilde{x}_D - \tilde{x}_R)]} \quad \text{(A-1)}$$

We want to show first that

$$\tilde{x}_D > \hat{x}. \quad \text{(A-2)}$$

From relationship A-1, relationship A-2 implies, after rearrangement,

$$\begin{aligned} &2\tilde{x}_D(\bar{x}^D - \bar{x}^R) + 2\beta\tilde{x}_D(\tilde{x}_D - \tilde{x}_R) \\ &\quad > \beta(\tilde{x}_D - \tilde{x}_R)(\tilde{x}_R + \tilde{x}_D) \\ &\quad + (\bar{x}^D - \bar{x}^R)(\bar{x}^D + \bar{x}^R) \end{aligned}$$

or

$$\begin{aligned} &(\bar{x}^D - \bar{x}^R)[2\tilde{x}_D - (\bar{x}^D + \bar{x}^R)] \\ &\quad + \beta(\tilde{x}_D - \tilde{x}_R)^2 > 0. \end{aligned} \quad \text{(A-3)}$$

The second term in relationship A-3 is positive. The first is also positive if

$$\tilde{x}_D > \frac{\bar{x}^D + \bar{x}^R}{2} \quad \text{(A-4)}$$

If $q = k$, relationship 16 implies the following inequalities:

$$\bar{x}^D - k > \frac{\bar{x}^D + \bar{x}^R}{2} > \bar{x}^R + k. \quad \text{(A-5)}$$

Condition 9 implies $0 < V^{DD} < 1$, which implies $\tilde{x}_D > \bar{x}^D - k$. Consequently, relationship A-5 implies relationship A-4. Thus relationship A-5 holds. By repeating the same procedure one obtains $\hat{x} > \tilde{x}_R$ if and only if

$$\begin{aligned} &(\bar{x}^D - \bar{x}^R)[2\tilde{x}_R - (\bar{x}^D + \bar{x}^R)] \\ &\quad - \beta(\tilde{x}_D - \tilde{x}_R)^2 < 0. \end{aligned} \quad \text{(A-6)}$$

Using relationships 16 and A-5 one can easily show that relationship A-6 holds. QED

Consider now the case of $q \neq k$. The inequalities corresponding to relationships A-3 and A-6 are as follows:

$$(\bar{x}^D - \bar{x}^R)[2\tilde{x}_D - (\bar{x}^D + \bar{x}^R)] + \beta(\tilde{x}_D - \tilde{x}_R)^2 > (q^2 - k^2)\mathrm{Var}(a) \qquad \text{(A-7)}$$

$$(\bar{x}^D - \bar{x}^R)[2\tilde{x}_R - (\bar{x}^D + \bar{x}^R)] - \beta(\tilde{x}_D - \tilde{x}_R)^2 < (q^2 - k^2)\mathrm{Var}(a). \qquad \text{(A-8)}$$

Relationships A-7 and A-8 show what is claimed in the text, precisely that Proposition 2 holds also for $q \neq k$ as long as q and k are not too different or var(a) is not too high. QED

Appendix B: Data

	Republican Share of Two-Party Vote		Growth Rate				
			Real GNP		Real GNP, 2d, 3d Quarters per Capita	Real Income per Capita	
Year	House	Senate	g	g_{-1}		i	i_{-1}
1950	49.959	50.534	8.539	.027	12.109	7.062	-1.995
1952	49.844	53.712	3.902	10.345	-.337	2.580	2.465
1954	47.272	43.665	-1.329	4.004	-.145	-1.249	3.571
1956	48.797	48.905	2.059	5.555	-.688	3.919	5.590
1958	43.603	44.027	-.766	1.670	4.199	-1.393	.376
1960	45.029	44.534	2.219	5.836	-1.917	1.600	3.729
1962	47.363	49.235	5.310	2.608	2.477	3.551	1.249
1964	42.498	43.247	5.336	4.108	2.380	4.266	2.332
1966	48.673	51.617	5.787	5.792	1.434	4.470	5.170
1968	49.078	49.189	4.152	2.852	4.002	4.242	3.224
1970	45.775	44.989	-.292	2.436	.948	.420	2.695
1972	47.335	53.679	4.980	2.839	5.054	5.138	1.808
1974	41.323	42.013	-.536	5.195	-2.951	-1.973	4.676
1976	42.754	44.635	4.887	-1.259	.781	3.530	-1.553
1978	45.678	48.590	5.290	4.669	7.072	3.648	2.979
1980	48.714	47.394	-.160	2.476	-5.694	-3.328	-.254
1982	43.782	44.039	-2.546	1.931	-2.009	-1.226	.098
1984	47.220	50.862	6.429	3.572	2.693	4.110	1.992

Notes

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1. See, however, the controversy between Beck (1982, 1984b) and Hibbs (1983). Hibbs (1987) has recently showed additional empirical evidence in favor of his view. For empirical evidence in European countries see Alt 1985.

2. For some results more favorable to Nordhaus see Haynes and Stone 1987 and Grier 1987.

3. Alternatively, the model could be closed by a quantity theory equation such as $m_t = \pi_t + y_t$, where m_t is the rate of money growth; in this case one could assume that the policy makers control money growth rather than inflation. The shortcut adopted in the text simplifies the notation without affecting in any way the results. We disregard the issue of the partial independence of the Federal Reserve. See Alesina 1988b on this issue in a related context. In addition, there are no shocks affecting the supply function as expressed in relationship 2. For a related model with supply shocks see Balke

1988. This author shows that economic shocks may affect voters' choices because each of the two parties is more suited to respond optimally to different types of shocks. These issues are not considered here.

4. This simplification eliminates the issue of time inconsistency of optimal monetary policy in rational expectations models pointed out by Kydland and Prescott (1977) and Barro and Gordon (1983). The time inconsistency arises if the unexpected inflation term enters the utility functions of the two parties. By not including output in the objective functions we avoid such a term. If $b^D \neq b^R \neq 0$, we would simply have that the time-consistent policies followed by the two parties when in office would imply a higher-than-optimal inflation rate. Our positive politicoeconomic results would be unaffected. Alesina (1987, 1988b) and Alesina and Sachs (1988) show extensively how to deal with this time inconsistency problem in two-party models.

5. Policy convergence can be achieved only if the electoral game is modeled as a repeated game, so that reputational mechanisms can be accounted for. See Alesina 1987, 1988a; Alesina and Cukierman 1987; and Alesina and Spear n.d. for repeated electoral games with ideologically motivated politicians and see Ferejohn 1986 and Rogoff and Sibert 1988 for repeated electoral games with purely office motivated politicians. We do not consider reputational mechanisms here, i.e., we consider every election as a one-shot game.

6. The specification adopted for the effect of Congress on policy making is perhaps more appropriate for the case of proportional representation, which does not apply to the U.S. Congress.

7. In a companion paper (Alesina and Rosenthal 1988) we show that this simple definition implies that the resulting equilibria are Von Neumann-Morgenstern abstract stable sets for Greenberg's (n.d.) dominance relation, a generalization of Bernheim, Peleg, and Whinston's (1987) coalition-proof Nash refinement. Greenberg's generalization enables us to treat the case of a continuum of voters.

8. Note that in the model with Congress the realization of the random variable a can generate unexpected inflation or deflation even in the second period of any administration.

9. Like Fair (n.d.), we report results based on the latest revisions of the data, which in the case of GNP have been quite substantial. However, such a choice is problematic. Kramer (1983) argues that one should model voters as basing their decisions not on changes in their total income but on changes in income that they attribute to governmental activity. However, one may argue that voters' assessments reflect the "real" economy rather than the "unrevised" announcements made by the government in the period before election. We have checked our analysis with respect to GNP data by using the CITIBASE series prior to the 1986 revisions. The results from the older series are somewhat more favorable to our argument.

10. We do not include presidential voting in this paper. We completely agree with Fair (1978), who, citing work by Lepper (1974), argues that it was unreasonable for Kramer (1971) to have estimated a model in which presidential and House elections were considered in a single model with constrained coefficients.

11. Our work uses real GNP (net per capita). Results for annual per capita growth rates are very similar to those presented here.

12. For the House, the F-test, which is equivalent to a two-tailed t-test here, is not significant, whereas the one-tailed t-test on the lagged variable is.

13. Like Fair (n.d.), we used $G_{t,q}$ ≡ real GNP for quarter q in year t from CITIBASE file GNP82 and $P_{t,q}$ ≡ quarterly population figures that Fair obtained from the Council of Economic Advisors. We computed g from the formula

$$g_t = 100[\left(\frac{G_{t,3}/P_{t,3}}{G_{t,2}/P_{t,2}}\right)^2 - 1].$$

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AN IMPRESSION-DRIVEN MODEL OF CANDIDATE EVALUATION

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We describe and test two process models of candidate evaluation. The memory-based model holds that evaluations are dependent on the mix of pro and con information retrieved from memory. The impression-driven model holds that evaluations are formed and updated "on-line" as information is encountered. The results provide evidence for the existence of stereotyping and projection biases that render the mix of evidence available in memory a nonveridical representation of the information to which subjects were exposed. People do not rely on the specific candidate information available in memory. Rather, consistent with the logic of the impression-driven processing model, an "on-line" judgment formed when the information was encountered best predicts candidate evaluation. The results raise both methodological and substantive challenges to how political scientists measure and model the candidate evaluation process.

Our primary aim is to develop and test two competitive models of the candidate evaluation process. We focus on information, our general perspective being the information-processing approach identified with Herbert Simon (1981, 1985; Newell and Simon 1972). All contemporary political science models of vote choice are *information-processing* models in that candidate evaluation is treated as a function of the mix of information (e.g., Brody and Page 1972; Campbell et al. 1960; Enelow and Hinich 1985; Kelley 1983; Kelley and Mirer 1974; Lazarsfeld, Berelson, and Gaudet 1944). However, none of them provide an accounting of the memory processes involved in candidate evaluation. Yet memory processes are critical to understanding candidate evaluation, because citizens are exposed to information (be it party affiliation, character, or policy stances) over an extended period of time, and some of that information must be stored in, and retrieved from, long-term memory when an evaluation or vote is necessary.

Despite the lack of explicit attention to memory in contemporary voting models, they appear to share the common implicit assumption that the direction and strength of candidate evaluation is in large part determined by the "evidence" available in memory. For example, Kelley and Mirer (1974) state the case for a memory-based model of vote choice: "The voter canvasses his likes and dislikes of the leading candidates and major parties involved in an election. Weighing each like and dislike equally, he votes for the

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candidate toward whom he has the greatest number of net favorable attitudes" (p. 574).

This memory-based model of evaluation makes two critical assumptions about human information processing. First, the information recalled by respondents is thought either to be a more or less veridical representation of the information they were actually exposed to (Kelley 1983) or—more reasonably—to reflect the salient information they were exposed to plus what they "reasonably" inferred about the candidates (Conover and Feldman 1986; Granberg 1985). Second—and from our perspective most critically—the model assumes that citizens base their evaluation on the evidence retrieved from memory. Moreover, the methodologies used to assess the validity of candidate evaluation models also reflect the importance of memory. The reliance on responses to the Survey Research Center (SRC) open-ended like-dislike questions and candidate issue proximity scales reflects assumptions that (1) the citizen is willing and able to conduct a search for relevant information in memory, (2) this search will generate a veridical or (more plausibly) a representative sample of the information to which the citizen was exposed, and (3) this available information is then used to inform an evaluation and vote choice.

We test the validity of these assumptions by examining two important aspects of the memory-judgment relationship as applied to the process of candidate evaluation. The first aspect deals with memory retrieval processes: How much of what kind of candidate information do citizens retrieve from memory? The key question being, Is the "configuration" of evidence represented in long-term memory a veridical mapping or (more plausibly) a representative sample of information to which the citizen was exposed, or is memory for a political candidate subject to systematic biases? One class of problems found to undermine seriously the "veridical" and "representative" memory models relates to *cognitive constraints* in the processing of information; that is, memory often reflects one's prior expectations, with the bulk of the empirical evidence strongly implicating the impact of prior beliefs on attention and memory processes (Fiske and Taylor 1984). A second issue we consider focuses on *evaluative constraints* in the processing of new information. Is the evidence culled from memory a reflection of rationalization processes that serve to "fit" the evidence to "match" one's current evaluation of the candidate? Again, the empirical evidence documents an evaluative bias in information processing, the basic finding being that people are prone to recall evidence that is consistent with current attitudes (Chaiken and Stangor 1987).

In addition to problems related to memory per se, we are concerned with the judgment process itself: How do citizens use whatever information is available in memory to inform an evaluation? To examine memory processes as they apply to candidate evaluation, we consider two competing information-processing models: the *memory-based* model of the judgment process and the "on-line," or *impression-driven*, model (Hastie and Park 1986; Lichtenstein and Srull 1987).

Memory-based processing. The memory-based model of candidate evaluation aligns with common sense in positing a positive relationship between memory and judgment. When a person is called on to make an evaluation, whether at the ballot box or in reply to an interviewer's request, it is reasonable to assume that the overall evaluation is based upon the actual mix of pro and con evidence available in memory. Common sense, unfortunately, is a fallible guide. A great deal of research indicates that there is typically little or no relationship between the blend of pro and con information available in memory and the direction or strength of evaluation (see Hastie and Park 1986 for a review). Apparently, memory-based evaluation is too cognitively taxing, relying on the effortful retrieval of specific information from long-term memory, the weighing of evidence, and then the computation of a summary evaluation, all common complaints leveled against rational choice models in general and issue voting models in particular (Herstein 1981).

Impression-driven processing. Given the failure to find unequivocal support for memory-based processing, a second class of models has been proposed. Impression-driven, or "on-line," processing occurs when a judgment is made as relevant information is encountered. A key concept in understanding the on-line model of evaluation is the notion of an "evaluation counter" or "judgment tally"—what Hastie and Park (1986) call a "judgment operator," and Wyer and Srull (1986) term an information "integrator." It is conceived as a counter in working memory that integrates new information into a "running tally" of one's current impression. What makes on-line processing so psychologically compelling is that unlike the memory-based models of the judgmental process, on-line processing does not tax the cognitive limits of the human information processing system unduly. When exposed to new information, people can operate naturally as "cognitive misers" by simply retrieving the evaluation counter from memory, updating this summary tally, storing the new value in long-term memory, and then in the name of cognitive economy "forgetting" the actual pieces of evidence that contributed to the evaluation. Thus it is that people can often tell you how much they like or dislike a book, movie, candidate, or policy but not be able to recount the specific whys and wherefores for their overall evaluation—a partial explanation, perhaps, for the paucity of specifics in the SRC open-ended like-dislike questions (Gant and Davis 1984).

For the memory-based model of evaluation, a summary judgment is thought to be "computed" from the specific memory traces recalled at the time the assessment is called for. To the extent that this represents an accurate portrayal of the evaluation process, it is arduous, time-consuming, and unreliable, requiring more effort and attentional resources than all but Downsian man could or would be willing to expend. On the other hand, the on-line model of the judgment process is psychologically realistic in proposing that when asked to voice an opinion, people typically retrieve their summary evaluation from memory, only dredging up specific memory traces when pressed to give reasons why they favor or oppose a candidate or issue. On-line processing appears to be more cognitively efficacious than memory-based evaluation, in that the individual is forming the judgment "on the fly" as evidence is encountered and updating the on-line evaluation immediately, without having to first store each piece of evidence in long-term memory and then later laboriously compute a summary evaluation from whatever memory traces are still available.

Clearly, people sometimes rely on their memory of likes and dislikes to inform an opinion, while at other times they can simply retrieve their on-line judgments. What conditions produce impression-driven or memory-based information processing? The critical mediating variable appears to be the individual's processing objective or "goal" when information is initially encountered. If, as is commonplace, the person acquires information with the explicit or implicit objective of forming an impression or making an evaluative judgment, impression-driven processing occurs. If, on the other hand, one's goal were to remember as much information as possible (as is typical of laboratory tasks but thought to be relatively rare in real world situations)—or, perhaps more likely, if one were exposed to information with no specific objective in mind and only later asked to render an evaluation of the target—then memory-based processing occurs: one's evaluation would be based on whatever bits and pieces of information are still available in memory.

At stake is how we model and interpret the correlates and pathways of candidate evaluation. When, as here, the citizen is conceived as a bounded rationalist, it is incumbent on us to construct models of candidate evaluation that do not require more capacity and processing skills than human beings are known to possess. To the extent that the evaluation of political figures is much the same as the evaluation of ordinary people by psychologists, our discipline's interpretations of the hows and whys of candidate evaluation and vote choice appear to demand too much of the voter, and our technique of asking respondents for their recollection of likes and dislikes (as well as their recollection of the candidates' position on the issue scales used to develop proximity scores) may well be misdirected.

In line with contemporary thinking in the cognitive sciences, we focus on the actual processes involved in decision making. From the cognitive perspective, a psychologically informed model of candidate evaluation must satisfy the constraints of "bounded rationality," the most important being that (1) the information in memory available for recall is rarely a veridical representation of what the citizen was exposed to but, rather, is cognitively constrained by inferences drawn from the stereotypic default values of one's representation; (2) the interpretation and recollection of new information is also constrained evaluatively, as people are prone to rationalize new information to make it better conform to their current evaluation; and (3) the relationship between what is available in memory and judgment is dependent upon the citizen's processing objective when information is encountered.

Experiment Overview

We set as our task an examination of the relationship between a person's memory for a candidate and his or her evaluation of that candidate. First, we examine the content of memory and its impact on the candidate evaluation, asking, What kind of information do people remember about a candidate under different processing conditions? What relationship exists between the mix of information available from memory and candidate evaluation? Next we turn to tests of the memory-based versus the impression-driven model of candidate evaluation. In line with a dynamic information-processing perspective, we incorporate some of the most important factors known to underlie the evaluation of political candidates. An implicit assumption of on-line processing is that "first impressions" (that is, the initial input into the judgment counter) should be a particularly powerful determinant of judgments. Within the electoral context, the political candidate's party affiliation is certainly a critical "first impression" factor and is accordingly incorporated into the information stream. Next, given the importance of "character" on the evaluation of political candidates, we systematically manipulated the second piece of information our subjects' were exposed to with an endorsement appraising them of the candidate's competence and integrity. Finally, we examine the impact of a candidate's policy statements on the evaluation process. Presumably, the voter's on-line integration of the candidate's issue positions will serve as a better explanation of the candidate evaluation process than will the information that the voter can later remember about the candidate.

Experimental Design and Method

A nonprobability sample of 422 nonstudent adults from Long Island, New York were interviewed by trained college students in partial fulfillment of a class project in the spring of 1987. Fifty-two percent of the subjects were male and 88% were white. The median family income was $40 thousand. The sample was well educated, with 68% reporting some college or a college degree. While the respondents in this sample were better educated and enjoyed more income than the average U.S. voter, they also expressed—more important for our purposes—similar levels of political interest and correspondingly low levels of political activity beyond voting. In terms of party affiliation, 29% of subjects reported being Republican, 35% Democrat, and the remainder Independent or of no party affiliation. The experiment proceeded in six stages, as summarized in Table 1.

Stage 1. Campaign brochure. The subjects were recruited to participate in a study that had as its ostensible aim the evaluation of a campaign brochure of a congressman seeking reelection. They first read the five-page campaign brochure, which described a Congressman Williams of Troy, New York as a long-time Republican who worked actively in the Republican party since the 1950s, had defeated the incumbent Democratic representative in 1980, and who had held the congressional seat since then. (Williams is a fictitious person; the subjects were not aware of this). Following this one-paragraph biography (which mentioned his Republican affiliation four times), the brochure cited 40 policy positions attributed to the congressman, each of them a declarative statement of the type, Congressman Williams "favors major cuts in federal spending on social programs." All 40 policy statements were in fact selected from the Lodge and Hamill (1986) inventory of policy statements. Thirty had been evaluated earlier as Republican items by a similar sample of New Yorkers, while the remaining 10 were policies characteristic of the Democratic party. Thus Congressman Williams's overall policy posture in the campaign booklet was 75% consistent

Table 1. Experimental Design

	Experimental Conditions			
		Impression-driven		
Stages and Tasks	Memory-based	Positive	Neutral	Negative
Campaign brochure				
Republican congressman	yes	yes	yes	yes
Endorsement by civic organization	no	yes (+)	no	yes(−)
Dimension for statements rating	readable/not	like/dislike	like/dislike	like/dislike
Distractor word test	yes	yes	yes	yes
Congressman evaluation	yes	yes	yes	yes
Policy position recall	yes	yes	yes	yes
Policy recognition	yes	yes	yes	yes
Final questionnaire				
Republican-Democratic rating of 60 policies	yes	yes	yes	yes
Like-dislike ratings of remaining statements (no. cases)	60	20	20	20
Partisan knowledge, political interest, party ID, ideology, demographics	yes	yes	yes	yes

and 25% inconsistent with his Republican affiliation.

For the experimental treatment we systematically varied the subjects' information-processing goal in reading the campaign brochure. Approximately one-quarter of the subjects ($N = 100$) were randomly assigned to the *memory-based* condition in which they first read the short biography describing Congressman Williams as a Republican and then read the 40 policy statements attributed to the congressman. To thwart the subjects in this condition from forming an impression of the candidate, they were told to rate each policy statement in terms of readability—how "easy or hard" "the statement [was] to understand" on a five-point scale. The stated rationale for this rating task was that the congressman's campaign staff wanted to know "whether the average American voter would be able to read and understand the campaign brochure."

The remaining 322 subjects were randomly assigned to one of three *impression-driven* conditions: a *positive endorsement* condition, in which the congressman was described by the "Troy Civic Association" as a hardworking, "in-touch," honest representative; a *negative endorsement* condition, where it was said Congressman Williams had a poor attendance and voting record and was "out of touch" with his constituency; or a *control* condition in which no endorsement by the Troy Civic Association was provided.[1] The subjects in these three conditions then read the same 40 policy statements in the campaign booklet as did those in the memory-based condition but evaluated the statements in terms of how much (on a five-point scale) they personally liked or disliked the congressman's policy position. The subjects in these three impression-driven conditions were also instructed to try to form an overall impression of the candidate while rating the issues ("Try to reach a judgment of how much you like or dislike the Congressman"). Our purpose here was to mimic the real world situation in which people are thought to integrate new information spontaneously into an evaluation (Hastie and Park 1986).

In short, in the three impression-driven conditions we attempt to trigger the impression formation process explicitly while also providing some subjects with information (civic association endorsement) designed to bias their initial impression in a positive or negative direction. We expect the impression formation process to guide subsequent memory and evaluation processes—hence the designation *impression-driven.* Conversely, the subjects who evaluate the policy statements at the syntactic level (in terms of readability) are not expected to form an impression and will, we expect, be forced to rely on information available in memory when called on to evaluate the congressman—hence the designation *memory-based.* Whether the set of 40 policy statements in the campaign brochure represents more than or less than a normal dose of exposure about a candidate is a moot question here. What is clear is that while the amount of information presented here about the congressman is well within the bounds of long-term memory, it is *well beyond the limits of short-term working memory.* Given this all-too-familiar predicament, people are expected to act "reasonably"—albeit not by the dictates of unbounded rationality. They take shortcuts, use heuristics, make guesses, and otherwise compensate for the fact that they must make do with less than all the information they were exposed to.

Stage 2. Distractor task. After having read and rated the statements in the campaign booklet, we had all subjects complete a 44-item *Readers' Digest* vocabulary test. This distractor test was designed to purge from short-term memory the policy statements made in the campaign brochure and simulate the more natural course of events where nonrelevant information intercedes between exposure and evaluation. Respondents were allowed five minutes to complete the multiple choice test. Verbal test scores did not contribute either substantively or statistically to the models of evaluation and are therefore dropped from further discussion.

Stage 3. Evaluation of the congressman.[2] Following the distractor test, two types of candidate evaluation were obtained. A global evaluation question asked subjects for an "overall impression" of the congressman on a five-point scale that ranged from "extremely favorable" to "extremely unfavorable." Subjects were also asked to rate Congressman Williams on the 24 trait adjectives used in recent SRC National Election Surveys. Two trait dimensions—competence (e.g., *hardworking, not qualified)* and integrity (e.g., *compassionate, dishonest)*—were evident from factor analyses and consistent with data obtained in prior research (e.g., Kinder 1986; Kinder and Abelson 1981; Markus 1982). Because the dimensionality of political character is not directly relevant to our concerns here and because the 24-item character index and global evaluation measure were highly correlated ($r = .73$), we combined the two into a single, equally weighted measure of candidate evaluation. Also included in this section were three checks on the endorsement manipulation. Examination of the response to these manipulation checks indicated that this information was attended to by the two groups receiving the endorsements.

Stage 4. The recall of policy statements. In a surprise recall test, the subjects were called on to recall "as accurately as possible" the actual policy statements that appeared in the campaign brochure.

Stage 5. The recognition of policy statements. Our major test of the processing models relies on the recognition data. Forty policy statements were presented in serial fashion, each followed by a five-point scale ranging from (1) "Yes, Congressman Williams definitely said this" to (5) "No, he definitely did *not* say this." Twenty of the policy statements were "old" items, that is, policies that actually appeared word for word in the campaign booklet and 20 were "new" policies, policy statements which the congressman never made. Half of the old and half the new statements were consistent with Congressman Williams' Republican party affiliation (policies judged characteristic of the Republican party) and half were inconsistent policies (identified with the Democratic party).

Stage 6. Final questionnaire. Following the recognition task, subjects were asked to characterize all 60 policy statements (the 40 from the brochure as well as the 20 new recognition test policies) on a five-point scale as characteristic of either the Republican or Democratic party. These ratings allow us to confirm the partisan direction of the policy statements. Following these partisan ratings, subjects in the memory-based condition were asked to rate all 60 policy statements on the like-dislike scale, while subjects in the impression-driven conditions, having evaluated the 40 statements earlier in stage 1, now rated just the 20 new policies.

As a measure of political knowledge all subjects were also asked to identify the party affiliation of contemporary political leaders and groups. The responses to the leaders-and-groups questions were combined with the partisan ratings of the 60 policy statements to form an additive index of partisan knowledge. Each correct response (defined by sample consensus opinion, which also corresponded to the authors' "expert" opinion) added a point to the knowledge score, whereas each incorrect response resulted in the loss of a point. *Don't know* or a missing response was scored as zero.

The final series of questions tapped political interest, party identification, ideological persuasion, conventional political activities, and demographics, all using the standard SRC formats.

Analytic Plan

We address two distinct questions in our empirical analyses. The first is concerned solely with memory: How much of what kind of information is available in memory? The second is concerned with the relationship between memory and judgment: How is the information available in memory used in candidate evaluation?

The analysis of the recognition data will allow us to determine the content of available memory.[3] The most stringent criterion would be a *veridical* representation—a "snapshot"—in which all 40 policy positions endorsed by the candidate were correctly recognized. After eliminating this unrealistic possibility, we turn to examine in depth whether the information available in memory is a *representative* subset of the candidate's message or whether it reflects a systematically distorted version of his positions. Two manifestations of biased memory are of interest to us here. The first is the extent to which voters create a *stereotyped* representation of the candidate. Looking at all 40 policies (those consistent with his Republican label and those inconsistent), is there evidence of a stereotyping bias in memory such that subjects correctly remember—and perhaps erroneously impute—more consistent (Republican) than inconsistent (Democrat) policies to the candidate? The second line of questioning examines the impact of the individuals' own policy preferences on memory for the candidate's policies in a direct test of *projection* bias (Brody and Page 1972; Conover and Feldman 1986; Markus and Converse 1979). Under what conditions are subjects likely to remember policy positions that they personally prefer accurately? Do subjects erroneously impute ("project") preferred issue stances onto the candidate?

Next, having determined the content of available memory, we move to incorporate memory variables into a process model of candidate appraisal. In keeping with this dynamic perspective on human information processing, we manipulate or measure "impression" variables at different points in time. The manipulated "first impression" variables consist of the candidate's partisanship and the positive, negative, or no *endorsement* of the candidate made by the fictitious Troy Civic Association in the brochure. Moreover, the subjects' ratings of their personal liking for the candidate's issue stances (under the impression formation instruction) forces them to revise their assessment continuously, presumably through the activation of the evaluation counter. The summary measure of the liking for the 40 campaign policies is used as an indicator of this updated impression (hereafter referred to as the *on-line evaluation).* Finally, the dependent measure is *candidate evaluation* (derived from the trait ratings and the "overall" impression question).

What Is Available in Memory?

We begin with the question, "How much of what kind of information from the campaign brochure leaves a recognizable trace in memory? Keep in mind that to measure available memory 40 policy statements were presented in the recognition task, 20 of them old items (a sample of statements that had actually been presented in the campaign brochure) and 20 of them new policies (statements that had not appeared in the brochure). Of the old and new items 10 of each type of statement were Republican, and 10 were Democratic policies. Subjects were required to indicate whether Congressman Williams

Table 2. Recognition Task Responses As a Function of Experimental Condition and Partisan Knowledge

	Experimental Condition			Partisan Knowledge			
Issues	Memory-based	Impression-driven	F(1,418)	Low	Medium	High	F(2,418)
Republican							
Old (in brochure)							
Hits[a]	.55	.69	40.22***	.62	.64	.71	7.97***
Misses	.23	.21	ns	.23	.22	.20	ns
New (not in brochure)							
False alarms[a]	.26	.30	4.91*	.27	.28	.30	ns
Correct rejections	.45	.52	6.30**	.47	.49	.56	6.88***
Democratic							
Old (in brochure)							
Hits	.64	.55	ns	.67	.63	.63	ns
Misses[a]	.18	.26	17.03***	.20	.24	.28	8.53***
New (not in brochure)							
False alarms	.15	.20	8.25**	.20	.19	.17	ns
Correct rejections[a]	.52	.60	5.47*	.51	.55	.68	17.33***

Note: The values are the proportion of responses of each type, calculated separately for type of issue (Republican or Democrat). Within each group of nonindependent responses (e.g., hits and misses) the data do not sum to 1.0 because *don't know* responses are not included. The values in the *F* columns are the main effect F-statistics and significance levels (ns = not significant).

[a] These responses are the components of the measure of stereotyping.

$^{*}p < .05$.
$^{**}p < .01$.
$^{***}p < .001$.

had or had not endorsed the policy in his campaign brochure.

Four types of responses are possible when analyzing recognition memory (Murdock 1982). There are two types of correct responses. A *hit* occurs when a subject responds, "Yes, he said that" to an old policy statement. A *correct rejection* occurs when a subject says *no* to a statement that had not appeared in the brochure. Similarly, there are two types of errors. A *miss* occurs when a subject responds "No, he didn't say that" to a statement that was actually presented in the brochure, and a *false alarm* occurs when a subject errs by responding "Yes, he said that" to a new item. For the present data, we tally the responses for the Republican and Democratic policy statements separately. The results are summarized in Table 2.

Table 2 reports the mean percentage of each type of response as a function of experimental condition—because there were no recognition differences among the three impression conditions, those data are combined—and level of partisan knowledge. The results clearly eliminate the implausible standard that recognition memory is a veridical mapping of the information in the campaign brochure: approximately 40% of all responses are errors or *don't know* answers.

Our primary concern is with how one's information-processing goal (memory-based versus impression-driven) affects memory for campaign information. As expected, recognition varied as a function of experimental condition. On three of the four correct response indicators (hits and correct rejections), subjects in the impression-driven conditions outperformed subjects in the memory-based condition. Particularly striking are the differences in hits for the party-consistent items (55% as opposed to 69%). This pattern is consistent with a large body of research showing that the simple act of forming an impression facilitates memory for information (Fiske and Taylor 1984). Recognition also varied as a function of level of sophistication. Consistent with prior research (Lodge and Hamill 1986), those with higher levels of partisan knowledge tended to be more accurate (in terms of hits and correct rejections) than those with lower knowledge levels.[4]

In sum, it is clear that the amount of policy information represented in memory differs significantly from that presented in the campaign booklet. Memory varies as a function of the processing goal adopted when originally exposed to the information, as well as the level of partisan knowledge. For those who treated the policy statements in the campaign brochure as mortar for constructing an overall impression of the candidate, the pattern of remembrances is significantly different from subjects in the memory-based conditions, who were restrained from forming an initial impression. The simple (and, we believe, natural) act of integrating the policy positions into an on-line candidate evaluation influences memory processes.

At this juncture—still focusing on the information available in working memory—let us explore the more provocative issue of whether what is available in memory is a representative sample of the encountered information or whether there is evidence of systematic distortions of reality. Two manifestations of biased memory can be examined systematically within this experimental design: stereotyping and projection.

Stereotyping. One heuristic device that individuals can use to deal with complex information in the social world is through the use of stereotypes (Hamilton 1981; Lippmann 1922; Taylor and Crocker 1981). *Stereotype* refers to an abstract knowledge structure representing beliefs about the members of social groups (Hamilton 1981). Once categorized, the individual "inherits" the representative characteristics of that group. The stereotypical cue evoked in the present study is *Republican,* as all subjects were told that the candidate was a Republican congressman.

The stereotype measure is based on the recognition responses that reflect the extent to which the congressman was perceived to be a prototypical Republican. Specifically, the measure consists of four independent components (as indicated in Table 2).

1. *hits* to old consistent items (correct recognition of Republican policies from the brochure)
2. *misses* of old inconsistent items (failure to recognize Democratic statements from the brochure)
3. *false alarms* for new consistent items (incorrect attribution to new Republican issues)
4. *correct rejection* of new inconsistent items (correct rejection of Democratic policies not in the brochure).

The stereotype measure was created by summing up the number of each of these four responses. The higher the score on this measure, the stronger the representation in memory of the congressman as a prototypical Republican.

The results of the 4 (processing conditions) × 3 (partisan knowledge) analysis of variance of the stereotype measure indicate that stereotyping was significantly more likely in the impression-driven conditions (average $M = 17.67$) than in the memory-based condition ($M = 14.49$,

$F[3,410] = 14.90$, $p < .001$). In addition, highly knowledgable individuals engaged in more stereotyping ($M = 18.88$) than did those with moderate or low amounts of partisan knowledge ($Ms = 16.42$ and 15.34, respectively, $F[2,410] = 25.77$, $p < .001$). The processing condition and knowledge variables did not interact. Analyses of the two separate dimensions of the stereotype measure—accurate responses and errors—yielded identical results. Thus, the highly significant stereotyping differences observed here are not due to either enhanced accuracy or to misperceptions alone but to both.

The most direct explanation for these differences in stereotyping is that individuals who see their information-processing goal as forming an impression of the candidate must integrate a large number of separate pieces of information—in this study 40 issues plus a few pieces of personal information—into a coherent whole. For these subjects *Republican* provides an initial cue on which to build this impression. Apparently, individuals with higher levels of knowledge have developed a partisan knowledge structure that is readily available for organizing this new information. In the process of forming an impression, some of the incoming information will nicely fit this schema—a Republican candidate espousing Republican policies—and be retained in memory (resulting in hits), while other pieces of information—Democratic policies—will not fit. These incongruent policies are more likely to be discarded because of their lack of match to expectations (resulting in the misses). Moreover, because stereotypes contain expected values (Taylor and Crocker 1981), when faced with the uncertain task of teasing out old from new issue statements, individuals rely on these "default" values to guide their judgmental process, thereby yielding a high false alarm rate to the new Republican issues and the high probability of rejecting new Democratic issues.

Projection. In addition to stereotyping (a bias based on expectancies derived from group membership cues), we also examine the extent to which our subjects' memories are biased by projection of personally preferred issues onto the candidate (see Krosnick 1988a for a review and critique of the projection literature). Evidence for projection typically rests on proximity scores (the difference between self-placement and candidate placement on issue preference scales). In contrast to this proximity approach, we are interested in the impact of personal preferences on the attribution of issue positions to the candidate (as evident from "Yes, he said that" responses in the recognition task). Given experimental control over the content of the campaign message and direct tests of memory accuracy, our projection measures are considerably different from previous operationalizations.

In order to assess projection, two ratio measures were computed for each subject: (1) the number of "liked"—or positively evaluated—statements attributed to the congressman in the recognition test, divided by the total number of policies the subject liked (labeled *positive memory*); and (2) the number of "disliked"—or negatively evaluated—policy statements attributed to the congressman, divided by the total number of disliked policies (*negative memory*). These two variables range from 0 to 1.00, with higher values reflecting a greater likelihood of retrieving each item type. The use of the ratios controls for initial differences in evaluations of the policies, resulting in greater precision in tests of memory biases.[5] Differentiating between positive and negative memory allows us to examine whether a projection asymmetry is evident in memory, specifically, whether positive projection (or assimilation) is more common and more powerful than negative projection (or contrast). In other words, the subjects should be more likely to retrieve from memory policies they personally

Table 3. Projection As a Function of Experimental Condition and Partisan Knowledge

	Experimental Condition			
	Positive Memory		Negative Memory	
Partisan Knowledge	Memory-based	Impression-driven	Memory-based	Impression-driven
Low	.42	.57	.36	.46
Medium	.40	.54	.37	.43
High	.43	.57	.40	.43

Note: The values are the proportion of positively and negatively evaluated issues attributed to the candidate in the recognition task. Higher values reflect a greater probability of attributing each issue type to the candidate.

prefer than policies they dislike, and, more importantly, the positive memory projection bias should be more strongly related to measures of candidate evaluation than negative memory projection bias (Krosnick 1988a).

Positive and negative memory were first analyzed by a 2 (processing condition) × 3 (partisan knowledge) × 2 (memory measure: positive or negative) analysis of variance, with repeated measures on the last factor. The results of that analysis are summarized in Table 3. Across all subjects, positively evaluated policies were more likely to be attributed to the congressman ($M = .48$) than were negatively evaluated policies ($M = .41$, main effect for memory measure $F[1,416] = 30.20$, $p < .001$). In addition, a main effect for processing condition indicates that the impression-driven subjects attributed a higher percentage of both positive and negative policies than did the memory-based subjects ($F\ [1,416] = 39.19$, $p < .001$). However, this main effect is qualified by a condition by memory measure interaction ($F[1,416] = 6.16$, $p < .01$), which indicates that the two processing groups differ more on the positive memory measure than the negative memory measure. Finally, partisan knowledge was not significantly related to the memory measures either as a main effect or in interaction with processing condition.

The increased likelihood of retrieving personally preferred issues provides preliminary evidence for a projection bias in memory. However, projection entails more than the individual's own issue preferences: affective orientations toward the candidate are also important (Brody and Page 1972; Markus and Converse 1979). In the present context, subjects who like the candidate should retrieve more positive policies and fewer negative policies than do subjects who dislike the candidate. In order to examine this relationship between biased memory and candidate evaluation, correlations between the appropriate measures were computed and summarized in Table 4.

Note that three different measures related to "candidate evaluation" are analyzed: the manipulated endorsement (coded 1, 0, and −1), the on-line evaluation of all policy statements, and the final candidate evaluation. In general, the results conform to expectations. In the case of the positive memory ratio, the more positive the on-line and the candidate evaluations, the greater the likelihood of retrieving personally preferred policies and attributing them to the candidate. Conversely, those with more positive evaluations (stemming from the endorsement, on-line evaluation, and candidate evaluation) were *less* likely to retrieve negatively evaluated policies. Finally, as would be predicted from the asymmetry hypothesis, the magnitude of the

Table 4. Relationships between Evaluation and Projection Measures

	Memory Availability	
Evaluation	Positive Memory	Negative Memory
Endorsement	.04	–.10*
On-line	.33***	–.10*
Candidate	.22***	–.10*

Note: Entries are correlation coefficients. The evaluation measures are coded so that positive values reflect the positive endorsement condition and more positive evaluations. Higher values on the memory measures reflect a greater probability of attributing positively and negatively evaluated issues to the candidate. Only subjects in the impression-driven conditions are included.

*$p < .05$.
**$p < .01$.
***$p < .001$.

relationship between candidate evaluation and memory is stronger for positive memory projection than for negative memory projection for two of the three measures (the exception being the weaker endorsement variable).

To sum up the results of the analyses of available memory, we find clear evidence that the information retrieved from memory is neither a veridical nor even a representative sample of the candidate's issues positions. Rather, the memory representation of the candidate's issue positions is systematically biased. The tendency to create a stereotypical memory representation is particularly pronounced for those in the impression-driven conditions, especially so for political sophisticates. Finally, to close the circle, individuals' own policy preferences also bias memory, as subjects are more likely to attribute issues they personally liked to a positively evaluated candidate.[6]

Candidate Evaluation: The Memory-Judgment Link

At this point we shift focus from memory per se to the candidate evaluation process itself. First, the memory-based and impression-driven models are compared to examine the conditions under which the mix of liked and disliked issues in memory predicts the direction and strength of candidate evaluation. We then proceed to develop and estimate a model of the candidate appraisal process.

The memory-judgment relationship. Recall that the crucial distinction between the two models in terms of understanding the memory-judgment relationship concerns the activation of the evaluation counter. When, as is typically the case (Hastie and Park 1986), an individual acquires information with the goal of forming an impression (our impression-driven conditions), the judgment counter is thought to be activated immediately, and the on-line summary evaluation is stored separately from the specific information used as its inputs. Under these conditions, there is no reason to expect a correlation between the specific facts retrieved from memory and either the strength or direction of judgment. On the other hand, if the information is acquired without the objective of forming an impression (our memory-based condition), the evaluation counter is not activated, and no summary evaluation is stored in memory. When later asked to make an evaluative judgment, the individual is forced to go into long-term memory to retrieve previously acquired information and then use what-

Table 5. Predicting Candidate Evaluation: A Comparison of Available Memory and On-Line Evaluation

	Experimental Condition			
		Impression-driven		
Variable	Memory-based	Positive	Negative	Neutral
Available memory	.20*	.13	-.01	-.01
On-line evaluation	.26**	.54***	.46***	.51***

Note: Entries are standardized regression coefficients. The variables are coded so that positive values reflect greater proportions of positively evaluated issues available in memory, more positive on-line evaluations, and more positive candidate evaluations.

*$p < .05$.
**$p < .01$.
***$p < .001$.

ever memory traces are presently available for computing the evaluation. Under this difficult condition, there should be a positive relation between candidate evaluation and the evaluative mix of information retrieved from memory.

To compare the two processing models optimally, a summary measure of the evaluation counter as well as a measure of the information available in memory is needed. We use the on-line evaluation (the sum of the likes [1] and dislikes [−1] for all 40 of the candidate's issue stances) as an indicator of the tally in the counter, as this score best represents the subjects' overall impression of the candidate after their exposure to all of the policies. The available memory measure is a ratio: the number of positively evaluated policies attributed to the congressman in the recognition test, divided by the total number of positive and negative policies attributed in the recognition test (Hastie and Park 1986). A ratio of .50 corresponds to an equal number of liked and disliked policies available in memory, a score greater than .50 indicates a greater proportion of liked policies, and a score less than .50 reflects a greater proportion of disliked policies.[7] These two measures were used to predict candidate evaluation. The regression analyses are summarized in Table 5.

The results are straightforward. As predicted, a significant relationship between available memory and candidate evaluation was obtained only in the memory-based condition. This result complements the findings in nonpolitical domains (Hastie and Park 1986; Lichtenstein and Srull 1987) demonstrating that the relationship between memory and judgment depends on processing objectives. Moreover, the on-line evaluation counter proves to be a much stronger predictor of candidate evaluation in the impression-driven conditions. Finally, the significant (albeit weaker) relationship between on-line evaluation and candidate evaluation in the memory-based condition suggests that we may have not been entirely successful in inhibiting the impression formation process among those subjects. (See Hastie and Park 1986 for a discussion of the difficulty in creating tasks that inhibit spontaneous impression formation).

Candidate evaluation. Our model of the candidate evaluation process includes the mainstay variables of party identification, ideology, and partisan knowledge, as well as the endorsement by the civic association (which was experimentally manipulated to have three values—positive, negative, and neutral—coded 1, −1, and

Table 6. Estimates of the Candidate Appraisal Model

Variable	Maximum Likelihood Beta	Standard Error	t-Value	Standard Beta
On-line evaluation				
Endorsement	1.28	.92	1.39	.07
Party identification	3.07	.84	3.66	.20***
Ideology	5.91	.87	6.79	.37***
"Old" issue memory				
Endorsement	.01	.01	1.53	.07
Party identification	-.00	.01	-.24	-.01
Partisan knowledge	-.01	.00	-2.87	-.13**
Ideology	-.02	.01	-1.62	-.09
On-line evaluation	.01	.00	11.35	.60***
"New" issue memory				
Endorsement	.04	.02	1.45	.08
Party identification	-.05	.02	-2.11	-.13**
Partisan knowledge	-.01	.00	-3.40	-.19***
Ideology	-.01	.03	-.54	-.04
On-line evaluation	.01	.00	4.18	.26***
Candidate Evaluation				
Endorsement	.29	.04	6.63	.30***
Party identification	-.03	.04	-.64	-.03
Ideology	.13	.04	2.89	.15**
On-line evaluation	.02	.00	6.59	.41***
"Old" issue memory	.21	.27	.76	.04
"New" issue memory	.16	.10	1.64	.08*

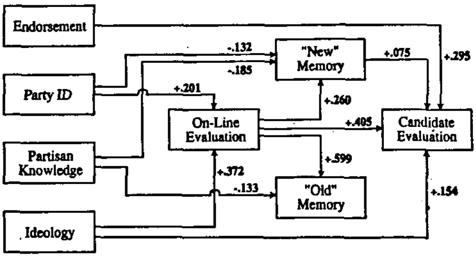
Note: Chi-squared (3 df) = 4.98, $p = .173$; goodness-of-fit index = .996; adjusted goodness-of-fit index = .953; root mean squared residual = 4.079.

*$p < .05$.
**$p < .01$.
***$p < .001$.

0). These exogenous variables are expected to be related to three components of the candidate evaluation process. The first is the on-line evaluation, which we assume is the key variable influencing subsequent memory and judgments. The second is the ratio measure reflecting the mix of positive and negative policies available in memory (as in Table 5). However, here we distinguish between old issues (the proportion of liked policies attributed to the candidate from those that actually appeared in the campaign brochure) and new issues (the proportion of liked policies attributed to the candidate that he never advocated, that is, false attributions of his policy stances). Finally, all of these variables are used to predict the dependent measure, candidate evaluation.[8]

The rationale for the model is straightforward. Party identification and ideology should be related to the endogenous variables because the candidate and his issues were portrayed as Republican. Partisan knowledge is included in the model because, as reported earlier, it reliably influences the representation of the candidate in memory. Most important, according to the dynamic processing logic underlying the impression-driven model, impressions formed early in the process guide both the subsequent encoding and retrieval of information, as well as evalu-

Figure 1. The Candidate Evaluation Model



Note: Entries are standardized regression coefficients. Only significant paths are shown.

ations. Therefore, we expect that the manipulated endorsement will be positively related to subsequent judgment and memory processes. The same logic applies to the effects of the on-line evaluation (where a positive relationship between the on-line counter and the subsequent memory and evaluation measures is expected). Although the two variables representing the information available in memory are not expected to be significantly related to candidate evaluation in the impression-driven processing conditions, particularly in comparison to the impact of the on-line evaluation (as seen in Table 5), we estimate those links in order to determine whether the old and new (i.e., false) memory measures differentially predict candidate evaluation.

The parameters obtained from the full information maximum likelihood estimation of the recursive model are reported in Table 6. The model itself is displayed in Figure 1, which includes all path coefficients that are significant at the $p < .05$ level (according to either one- or two-tailed significance tests, as dictated by the logic of the processes). Consider first the effects involving party identification, ideology, and partisan knowledge. As expected, both party identification and ideology exert significant effects on the on-line evaluation. Republicans and conservatives feel more positive about the candidate's issue positions than do Democrats and liberals. Also, ideological persuasion has a significant direct influence on candidate evaluation.

Partisan knowledge is inversely related to both memory variables. Individuals with high levels of partisan knowledge were less likely to retrieve a high proportion of old issues that they personally liked, and also less likely to falsely project their preferred new issues onto the candidate than were individuals with lower levels of partisan knowledge. The intriguing relationships that exist between partisan knowledge and the various memory measures are worth noting. Whereas stereotyping (an expectancy-based, nonaffective bias) occurs more frequently among those with high levels of partisan knowledge (Table 3), personal policy preferences have a stronger impact on memory among the less-knowledgeable. This pattern of results is consistent with recent findings indicating that those with high levels of political expertise are more likely to draw on partisan schemata when making political inferences (Conover and Feldman 1986; Hamill, Lodge, and Blake 1985). The overall effect is an increased likelihood of creating a stereotypical representation of the candidate. On the other hand, those with low levels of political expertise are more likely to rely on personal preferences, resulting here in a greater likelihood of projecting their policy preferences onto the candidate.

The pathways depicted in Figure 1 confirm our expectations about impression-driven processing. First, the endorsement had a direct and highly significant impact on candidate evaluations and is also associated with more positive on-line evaluations (although this link does not reach an acceptable level of significance).

Consider now the relationship between the memory and judgment variables. Note the critical result: the highly significant direct link between on-line evaluation and candidate evaluation (a replication of the simple regression effects provided in Table 5 within the context of the complete appraisal model). Moreover—as predicted—*no* link exists between preferences for the candidate's actual issue stances that are available in memory (old issue memory) and candidate evaluation. There is, however, a modest but reliable link between evaluation and "memory" for the issues *falsely* attributed to the candidate. In other words, whereas memory for the candidate's actual policy positions has no discernible effect on candidate evaluation, the false belief that the candidate voiced personally preferred stands is directly linked to evaluation. We interpret this linkage as evidence of projection bias in the candidate evaluation process. Nevertheless, it should be noted that the magnitude of this relationship is relatively weak, consistent with such effects as reported by Markus and Converse (1979).

According to the observed chi-squared statistic (χ^2 [3 df, N = 319] = 4.98, p = .173), the model provides a reasonably good fit to the data. The adjusted goodness-of-fit index coefficient is .953, indicating that the majority of the variance and covariance is accounted for by the model. A comparison of the estimated model to an alternative provides a stronger method for assessing model validity (Anderson and Gerbing 1988). To test directly our assumption that on-line evaluation is the critical component of the candidate appraisal process, we reestimated the model after eliminating the on-line evaluation-candidate evaluation link. The difference between the chi-squareds obtained from the full and "nested" models was significant (chi-squared difference [1 df, N = 319] = 40.77, $p < .001$; adjusted goodness-of-fit index of the alternate model = .70), indicating that the model is significantly improved by considering the path between the on-line tally and candidate evaluation.

General Discussion

Because citizens are exposed to political information over extended periods of time but are only periodically called on to voice an opinion or cast a ballot, a psychologically realistic model of candidate evaluation must be able to account for what information the citizen has available in memory as well as how the mix of evidence in memory contributes to evaluation. In this study we examine the validity of the assumption of a direct memory-judgment relationship underlying contemporary models of candidate evaluation and (finding it wanting) propose an alternative method of the candidate evaluation process. The results raise both methodological and substantive challenges to how we measure and model the candidate evaluation process.

Focusing first on memory—on what information about the candidate is stored in memory and ostensibly available for making an evaluation—two findings emerge. First, people whose goal is to form an impression of the candidate tend to constrain their memory representation cognitively by attributing to the candidate policy positions consistent with their own stereotypical preconceptions. Second, people tend to constrain the evidence in memory evaluatively by attributing to favored candidates policy positions they personally like. The effect of both biases is to render the mix of evidence in memory a nonveridical representation of the information subjects were actually exposed to.

The primary thrust of this research was directed toward the evaluation process itself: how information is integrated into a summary evaluation. The major findings of this study challenge the assumption of a direct relationship between memory for a candidate's policy statements and evaluation of that candidate. We find—in direct contrast to the common sense assumption underlying political science models of candidate evaluation—that when called on to form a general impression of the candidate, people do not rely on the specific issues available in memory. Rather, the evidence suggests that the "judgment counter," activated when information was initially encountered, is retrieved and used to render an evaluation.

The results may appear paradoxical on two levels. First, although two theoretically meaningful memory biases (stereotyping and projection) were identified, neither is a strong predictor of candidate evaluation. There was no evidence that stereotyping is related to evaluation,[9] and the impact of projection is relatively weak (in comparison to the magnitude of the relationships between candidate evaluation and the on-line evaluation, endorsement, and ideology). However, this pattern is indeed consistent with the logic of the impression-driven processing model: the on-line tally should be—and is—a stronger determinant of evaluation than memory variables. We are not concluding that issues involving the encoding, representation, and retrieval of political information are unimportant. Rather, we would like to make the case for greater precision in specifying the conditions under which a strong relationship between memory biases and candidate evaluation is likely to occur.

More important, these results are in strong contrast to the majority of studies, which find a strong positive relation between a respondent's specific likes and dislikes about a candidate and evaluation (e.g., Kelley 1983; Kelley and Mirer 1974; Lau 1986; Miller, Wattenberg, and Malanchuk 1986). The numerous methodological differences between this study and investigations that rely on the NES like-dislike measures compel caution. A tentative resolution of these disparate results requires a consideration of the nature of the like-dislike survey responses. A substantial proportion of the responses to the open-ended probes are *not* specific personal or policy attributes but rather "diffuse evaluative judgments" (Stokes and Miller 1962; see also Gant and Davis 1984), corresponding to our notion of retrieval of the on-line tally from memory. Moreover, strong partisans (who, we suppose, are most likely to be chronic impression-driven processors because of their interest in politics) are more likely to respond to the open-ended probes with nonspecific and generally affective expressions, whereas those with but weak partisan feelings are more likely to provide explicit, detailed responses (Gant and Davis 1984). In addition, Kelley and Mirer (1974) report that the predictive accuracy of the like-dislike index actually *declines* as the number of specific likes and dislikes reported by the voter increases, a finding that is consistent with our contention that specific memory traces are not related to evaluation.

In sum, what we see as our contribution to this literature is the idea that voters do not typically rely on their memory for specific issues to inform their evaluation but instead call up their summary tally when asked for an evaluation. This on-line processing is, at least in its broad strokes, psychologically realistic in placing minimal information-processing demands on voters. Indeed, the model suggests an even simpler decision rule than that thought to guide "the simple of act of voting" (Kelley and Mirer 1974), namely, *retrieve the on-line tally.* To the extent that this is an accurate portrayal of the actual evaluation process, it challenges the logic of asking respondents their likes and dislikes as indicators of the reasons underlying their vote choice. While political scientists have long been aware that voters often forget and fabricate their past partisanship, attitudes, and voting behavior, this concern should be extended to the information voters report as underlying evaluations of political candidates.

It is increasingly evident that citizens are not well informed and cannot engage in the computations required by most theories of voter rationality. We add to this the caveat that candidate memory is distorted by systematic biases that stem directly from impression-driven processing. However, from a cognitive perspective, these consequences of impression-driven evaluation are a natural and (dare we say) "reasonable" way to compensate for the severe limitations on human information processing that render models of unbounded rationality psychologically unrealistic.

Notes

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1. Note that the absence of an endorsement means that the control-condition subjects received the same information about the candidate as did the subjects in the memory-based condition. Those two conditions differ only in processing objective.

2. At this juncture in the experiment we included a task order manipulation. Half the subjects (regardless of processing condition) were asked for their evaluation of Congressman Williams immediately after the five-minute distractor task, followed by the recall and recognition tasks (as outlined in the text and Table 1). This group probably best represents the normal course of events, where the citizen is first exposed to information and later called on to make an evaluation. The other half of the subjects completed the two memory tasks before giving their evaluation. However, because preliminary analyses did not reveal any reliable differences due to the task order manipulation, all reported analyses collapse the data across this factor.

3. Use of the recognition data is necessary to consider the first issue of memory accuracy. For the sake of consistency and brevity we rely on the recognition data, rather than the recall data or some combination of recognition and recall, for all subsequent analyses. The analyses reported in Tables 3–6 were also done substituting comparable measures derived from the recall data for the recognition-based measures. The conclusions drawn from all the recall-based analyses correspond to those reported in this paper. A complete summary of the recall analyses is available from us on request.

4. Signal detection theory provides a summary measure of recognition accuracy, d', which assesses memory "strength" as a function of the ratio of hits to false alarms (Murdock 1982). Analyses of d' for these data (conducted after dichotomizing the five-point recognition responses and considering the Republican and Democratic issues separately) confirm our conclusions. The impression-driven subjects were significantly more accurate than the memory-based subjects (*Fs* [3,418] = 10.74 with $p < .001$ and 2.54 with $p < .05$, for the Republican and Democratic issues, respectively). Similarly, subjects high in partisan knowledge were significantly more accurate than subjects with low or moderate amounts of partisan knowledge (*Fs* [2,419] = 12.39 with $p < .001$ and 4.73 with $p < .01$).

5. The stereotyping and projection bias measures are independent ($r = .04$ for stereotyping and positive projection memory, $r = .15$ for stereotyping and negative projection memory).

6. We do not mean to imply that stereotyping and projection are the only biases that have an impact on political candidate memory. At least two other biases could be contrasted with the "veridical memory" model. First, we suspect that more salient or important issues have an advantage in memory over less important issues (Krosnick 1988b). In addition, the order in which information is received typically has an impact on memory such that more recent information has an advantage over older information (Anderson and Hubert 1963; Dreben, Fiske, and Hastie 1979). We are not able to examine the impact of salience and order in the context of this experimental design. See, however, McGraw, Lodge, and Stroh 1988 for an extension of the present work examining salience and order biases.

7. This available memory measure is similar, but not equivalent, to the projection measures used in the analyses reported on p. 408 and Table 3. The projection measures reflect the *probability* of retrieving either liked and disliked issues, controlling for initial evaluations of the issues when first encountered in the brochure. The memory availability measure reflects the *mix* of liked and disliked issues available in memory. The correlations between the available memory ratio, and positive-negative projection memory ratios are .50 and −.39, respectively.

8. At this point, we focus exclusively on the impression-driven processing condition and do not estimate the model for the memory-based condition. Inclusion of that condition was critical for understanding the impact of processing objectives on memory, as well as the relationship between available memory and judgment. However, the impression-driven conditions represent the most common and realistic information-processing mode, whereas the memory-based condition was purposefully artificial. Therefore, the impression-driven condition data provide the optimal circumstances for estimating the parameters of the candidate appraisal model.

Moreover, the stereotype measure was not included in the candidate appraisal model. As operationalized here, stereotype is purely a cognitive measure, and there are no a priori reasons to expect that this affect-free variable will (by itself) predict an evaluative judgment. Consistent with this reasoning, preliminary analyses revealed that stereotyping was not related to candidate evaluations. For the sake of parsimony, stereotyping was omitted from the complete model estimation.

9. See n. 8.

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A SCHEMA-BASED APPROACH TO MODELING AN AFRICAN-AMERICAN RACIAL BELIEF SYSTEM

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We use a cognitive schema-based approach to model an African-American racial belief system, showing the content of racial belief systems in a national sample to be associated with the individual's degree of socioeconomic status, religiosity, and exposure to black media. We find that African-Americans with a higher socioeconomic status are less supportive of black political autonomy and that they feel themselves more distant from black masses and black elites than do those of lower socioeconomic status. Religiosity, while unrelated to black autonomy, strengthens closeness to black masses and black elites. Black television—and, to a much lesser degree, black print media—had a consistent impact on the racial belief system. We conclude by discussing the complexity of the African-American racial belief system and potential directions for future work.

Race has been a major determinant of African-Americans' life chances. The predominance of race as a factor in black life is such that to a very large degree blacks and whites exist in two separate worlds. The Kerner Commission's findings of two decades ago that black and white citizens live in two worlds, separate but unequal, has continued to be valid right up to the present (Dawson 1986; Farley 1984; U.S. Riot Commission 1968). Several researchers have suggested that the current and historical social experiences of African-Americans have led to the formation of black consciousness and a racial belief system (Dawson 1986; Miller et al. 1981; Robinson 1987).

We consider one aspect of a general model of black group consciousness and present a formal model of black racial identity and its social, cognitive, and environmental antecedents. Specifically, we formulate a model of black racial belief system based on the interaction of the cognitive structures (cognitive schemata) that comprise it. We discuss how belief systems in general and this belief system in particular help process, constrain, and bias one's interpretation of reality and influence social and political behavior. We then show how socioeconomic status, individual religiosity, and black media influence the content of individual African-Americans' belief systems. As we develop the model, we discuss how this belief system might influence the social and political behavior of African-Americans.

Despite recent assertions that race is less salient in African-American social and political lives than it was before (Wilson 1980), we believe that there is ample

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evidence of the importance of race in influencing black life chances and in helping to structure African-Americans' political beliefs and choices. Recently Reynolds Farley has shown that U.S. residential patterns are still dominated by patterns of racial segregation—rich blacks do not live with rich whites, and poor blacks do not live with poor whites.[1] Large gaps in both unemployment rates and degree of wealth between blacks and whites, a once-again-widening income gap, and truly catastrophic black infant mortality rates all help to demonstrate the difference in economic and social reality between blacks and whites.[2] These objective differences are reflected in the political gulf between black and white citizens. African-Americans remain concentrated on the far liberal end of the U.S. political spectrum, continue to be overwhelmingly Democratic even in the face of a recent conservative trend in U.S. politics, and emphasize egalitarian aspects of democracy as opposed to aspects of democracy more centered on liberty and a laissez-faire state (Brady and Sniderman 1985; Dawson 1986; Nie, Verba, and Petrocik 1979; Verba and Orren 1985).

These differing worldviews, when combined with the often-forced separation of African-Americans in all aspects of U.S. society, have facilitated and increased the salience of African-Americans' racial identity. The question of the racial identity of African-Americans has been tied to discussions of political participation (Danigelis 1978; Gutterbock and London 1983; Miller et al. 1981; Shingles 1981; Verba and Nie 1972), political preferences (Brady and Sniderman 1985), and support for the president (Dawson, Brown, and Cohen 1988). Though an African-American racial belief system has been widely discussed in the literature, a formal description of it has rarely been attempted.[3] We first discuss some of the critical issues in the belief system literature that apply to our study.

Belief System, Schemata, and African-Americans

Belief systems are cognitive structures that tend to facilitate and constrain the work of schemata, namely, the processing, storing, and organizing of information acquired from the social environment. Belief systems also help to focus individuals' perceptions of social reality; bring attention to the complementary or conflicting nature of relevant actors' goals; and influence planning, counterplanning, and social behavior (Carbonell 1981). While cognitive schemata actively process and store information and generate expectations about future events and actions (Conover and Feldman 1980), belief systems help the social participant access and process information and make decisions heuristically. Carbonell argues that belief systems target the perception of the understander toward certain characteristics of the actors and the environment and filtering out other characteristics (Carbonell 1981).

The African-American belief system explored here would come into being over time, and the strength of the components of the belief system—the cognitive schemata—would vary across individuals according to their status in society as a whole, exposure to information sources both within and without the black community, and level of ability and training in processing social information. The replication of the belief system over time is reinforced by the socializing effects of such formal and informal black institutions as the black church and kinship networks, social organizations and institutions of the black community.

The Elements of the African-American Racial Belief System

Much of the extensive research on African-American racial group-based attitudes and beliefs have tended to focus on the centrality of group identification and consciousness. For the most part, the research question addressed has been how strong psychological attachment to the racial group affects the behavior of group members. More recently the study of group consciousness has been linked to the concept of the intergroup conflict. To the extent that group conflict (in our case racial conflict) exists, group consciousness will be strengthened (Bobo 1983; Dawson 1986; Robinson 1987).

We argue that racial identity is a multidimensional concept. Each dimension is conceived as being based on one of the cognitive schemata. We assume that the African-American racial belief system is composed of multiple cognitive schemata and that these schemata are distinctive but interrelated cognitive constructs that vary across individuals in their degree of intensity within the black population. This belief system is the cognitive manifestation of the often conflicting and different political and social heritages of white and black citizens. Five cognitive schemata are conceptualized: (1) black autonomy, (2) closeness to the black masses, (3) closeness to black elites, (4) the propensity to adopt positive stereotypical beliefs about African-Americans, and (5) the propensity to adopt negative stereotypical beliefs about African-Americans.

Black racial consciousness has often been conceptualized in terms that emphasize black autonomy—black awareness of their own rational group interest and opposition to the interests of other groups (Allen and Hatchett 1986; Miller et al. 1981; Shingles 1981)—and we further argue that African-Americans use group utility as a "low-cost" proxy for individual utility. This aspect of black consciousness taps the notion that blacks need to be aware of the group's plight. In addition, we need to examine racial beliefs that may tap feelings as to how the group is to obtain social, economic, and political power. Therefore, a belief that blacks should build political, social, and economic institutions based on the cultural values and interests of the group should be accepted as an underlying norm of this black belief system (Aberbach and Walker 1970; Gurin and Epps 1975). The study by Gurin and Epps (1975) on black postsecondary institutions during the civil rights era reveals that this cognitive structure has both political and cultural elements. Two of the cultural factors they assert to have an effect are African-American identity and identification with African symbols. A common theme running through these works is a strong commitment to racial solidarity to enable blacks to become an empowered and autonomous group. We will include both political and cultural aspects of black autonomy in our construct.

Important work done by Allen and Hatchett (1986) indicates that one can conceptualize and measure racial group closeness, or identification, as having at least two dimensions: (1) identification with the masses of black people, or mass group identification, and (2) identification with potential black leaders, or elite group identification. Both constructs are included in our model.

The last two constructs are schemata representing positive and negative stereotypical beliefs about African-Americans. Positive stereotypical beliefs means freedom from the notion that blacks lack legitimate social meaning. Thus, a sense of group pride and positive affective posture toward blacks is an important dimension of black consciousness. We need a construct that depicts the positive perception of blacks in relation to black identification in order to improve our knowledge of the different components of the belief system. On the other hand, African-Americans have been associated with negative personal characteristics throughout U.S. history. The very essence of the psychological component of racism is the belief in the inherent inferiority of one race as compared to another. This "imputation of inferiority" continues to be manifested in the political system (Greenstone and Peterson 1973) as well as in the media (Banks 1977; Weigel, Loomis, and Soja 1980). The construct of positive, as well as negative, stereotypical beliefs about African-Americans are specified explicitly in our model of the belief system.

The five constructs included in our model incorporate what we believe are three major components of racial consciousness. The black autonomy schema represents the component of racial consciousness that signifies the distinct cultural and political identity of African-Americans. The closeness schemata represent an affective component of racial consciousness, in which the individual places him- or herself in relation to the major strata of the group. The stereotype schemata indicates the degree to which the individual has internalized and adopts both the negative stereotypes imposed by the dominant society and the positive stereotypes often created by dominated groups to assert their worth in a hostile society.

Social Status, Information Sources, and the African-American Racial Belief System

Social status, religion, and the black media are major influences on, and shapers of, the African-American racial belief system. We discuss each in turn to gain a fuller understanding of this interaction.

Socioeconomic Status and the African-American Racial Belief System

Race and socioeconomic class remain the two major social factors that determine individual African-Americans' place in society and life chances. However, these two factors remain intertwined, objectively and psychologically. The economic position of African-Americans still remains linked to race, as the racial differences in unemployment and poverty rates and occupational structures attest (Farley and Allen 1987; Reich 1981). The psychological connection between race and class among African-Americans has also been demonstrated (Jackman and Jackman 1983; Kilson 1983). Thus we would expect individual socioeconomic status to be directly linked to the racial belief system.

As blacks move up the social ladder they feel less close to the masses of poorer black people, according to many theorists studying social stratification in the black community (Wilson 1980). At the same time, Jackman and Jackman report that the black middle class has relatively low affect toward members of their own groups (Jackman and Jackman 1983). Further, the Jackmans report that lower-status blacks have affective barriers against both whites and middle-class blacks. Kilson in particular discusses the tension-filled position of the black middle class, who are totally at home neither among the black masses nor in mainstream white society (Kilson 1983). Consequently, following the prediction of Kilson, we expect socioeconomic status to be negatively associated with closeness both to black elites and the black masses.

While higher socioeconomic status is associated with greater distance from other blacks, it has also been argued that increased status is associated with a rosier picture of the group as a whole. More importantly, Shingles (1979) has argued convincingly that middle-class blacks would oppose negative stereotypes both because they would be as damaging to middle-class blacks as to their less affluent brethren and because higher levels of education are associated with more experience and capability in combating racist and other negative stereotypes. Consequently, we would expect to find socioeconomic status negatively associated with negative stereotypical beliefs—and positively associated with positive stereotypical beliefs about African-Americans. Finally, it is argued that education and increased status are associated with a more secular and skeptical outlook on life. Therefore, we expect socioeconomic status to be negatively associated with religiosity.

We modeled socioeconomic status as a latent construct composed of occupational prestige, education, and income. Theoretically, the predictions concerning socioeconomic status in relation to the rest of the model are as follows. Those with more resources, particularly education, should rely more on black print media and watch less black television. These relationships are well established in the literature (Allen and Bielby 1979a, 1979b). Furthermore, the higher one's status, the more one is presumed to be integrated into the mainstream of U.S. life (Dawson 1986; Kilson 1983; Wilson 1980). Consequently, we expect to find a negative relationship between socioeconomic status and the more separatist schema of black autonomy.

The hypotheses that relate socioeconomic status to the African-American racial belief system are[4]

1. Socioeconomic status (SES) has a positive influence on exposure to black print media.
2. SES has a negative influence on exposure to black television.
5. SES has a negative influence on black autonomy.
6. SES has a negative influence on closeness to the masses of African-Americans.
7. SES has a negative influence on the closeness to black elites.
8. SES has a positive influence on propensity to adopt positive stereotypical beliefs about African-Americans.
9. SES has a negative influence on propensity to adopt negative stereotypical beliefs about African-Americans.

The Role of Religion in African-American Racial Belief System

Given empirical, historical, and contemporary evidence that black churches continue to play important and influential roles in the social, economic, and political life of African-Americans (Brown and Jackson 1988; Drake and Cayton 1970; Morris 1984), we posit that those who have strong religious commitments will be integrated into social networks and the black community. Therefore, those who express a strong sense of religiosity will have a strong sense of racial identification and consciousness. Moreover, we predict that religiosity will have a positive relationship with exposure to black print and broadcast media. This is based on the assumption that those who are part of a black religious community will perceive black media outlets as providing positive images and information about the racial group. In this study, a religious community is conceptualized as including members who share a common set of religious beliefs, values, and traditions and who engage in regular social interaction (see Wald 1987). Because of this interconnectedness among persons of a religious group, a sense of common identity will emerge (see Cone 1986; Dawson 1986; Smith 1986).

Although religious socialization among African-Americans results in believing that God does identify with the poor and oppressed, it does not foster an uncritical evaluation of African-Americans. We argue that religiosity is also associated with negative stereotypical beliefs, as humanity is seen as sinful and the black church is involved in extensive activity to correct the social problems of black communities. This set of hypotheses is based on seeing the black church as promoting a creed similar to that of liberation theology in the Third World (Cone 1986; West 1982).

3. Religiosity has a positive influence

on exposure to black print media.

4. Religiosity has a positive influence on exposure to black television.
10. Religiosity has a positive influence on black autonomy.
11. Religiosity has a positive influence on closeness to the masses of African-Americans.
12. Religiosity has a positive influence on the closeness to black elites.
13. Religiosity has a positive influence on the propensity to adopt positive stereotypical beliefs about African-Americans.
14. Religiosity has a positive influence on the propensity to adopt negative stereotypical beliefs about African-Americans.

The Black Media and the African-American Racial Belief System

We want to construct a model that shows the influence of the black media on dimensions of racial identification and consciousness. The black media are seen as a filter of African-Americans' information sources pertaining to the general status of African-Americans both as an autonomous group and in relation to the dominant society. As such, black media should play a significant role in determining the content of the African-American racial belief system.[5]

Two very important issues are why and how the black media, as sources of communication and potential transmitters of information about the black culture, either reinforce or crystalize the cognitive schema, or mental images, of social reality. Historical evidence from the nineteenth century implies that the black print media, particularly during the antislavery period, served two important functions. First, the cry for independent black newspapers to depict positive traits of the group and show the need for self-help organizations was meant to instill a sense of group pride and efficacy among free blacks. Thus, the black media acted as agents of social change. The media's second function was to crystalize black racial consciousness.

The empirical evidence about the ability of black print media to crystalize racial identification and consciousness is somewhat mixed. Allen and Bielby (1979b) find that racial identification is not correlated with reading such established black print media as *Ebony* but that it is positively correlated with exposure to such nontraditional black print media as *Black Scholar*. Yet in a later study, in which the type of black print medium was not categorized, Allen and Hatchett (1986) find that exposure to black media is positively correlated with positive perceptions of the group. These findings suggest that the black print media in general may play an important political socialization role in the reinforcing of positive images of the group. In general, we expect to find a positive relationship between black print media and all of the belief system schemata except strength of negative stereotypical beliefs, where we expect to find a negative relationship.

Exposure to television as compared to other mass media may be more important in the communication and transmission of cultural orientations. Television is said to cultivate the dominant culture (Gerbner, Morgan, and Signorielli 1980). The treatment of blacks by television has been largely stereotypical and has presented blacks in subordinate roles relative to whites (Comer 1982; Pierce 1980). While the number of blacks to appear on television has increased, numerous content analyses (e.g., Banks 1977; Weigel, Loomis, and Soja 1980) have documented the less-than-favorable images presented of blacks as a group.[6]

The question used to gauge black television exposure was, "How often do you watch black television programs?" This might be interpreted by the respondent to mean either programs written from a white perspective with blacks as main characters (primarily entertainment shows, e.g., black situation comedies) or programs focusing on topics relevant to blacks as a group and written from a black perspective, such as programs on black public affairs or black culture. We assumed that the majority of the respondents will interpret the question in the latter sense; thus, black television is thought to have a positive effect on black racial beliefs (see Allen and Hatchett 1986). Given this interpretation as to blacks and black media sources, we propose the following hypotheses about the influence of exposure to black media on the African-American racial belief system.

15. Black print exposure has a positive influence on black autonomy.
16. Black print exposure has a positive influence on closeness to the masses of African-Americans.
17. Black print exposure has a positive influence on the closeness to black elites.
18. Black print exposure has a positive influence on the propensity to adopt positive stereotypical beliefs about African-Americans.
19. Black print exposure has a negative influence on the propensity to adopt negative stereotypical beliefs about African-Americans.
20. Black television expsosure has a positive influence on black autonomy.
21. Black television exposure has a positive influence on closeness to the masses of African-Americans.
22. Black television exposure has a positive influence on the closeness to black elites.
23. Black television exposure has a positive influence on the propensity to adopt positive stereotypical beliefs about African-Americans.
24. Black television has a negative influence on tne propensity to believe in negative stereotypical beliefs about African-Americans.

In the next section, we will present the model used to test the various hypotheses proposed above.

Methodology

The 1980 National Survey of Black Americans, conducted by the Program for Research on Black Americans at the Survey Research Center at the University of Michigan during 1979 and 1980, provided the data for this project.[7]

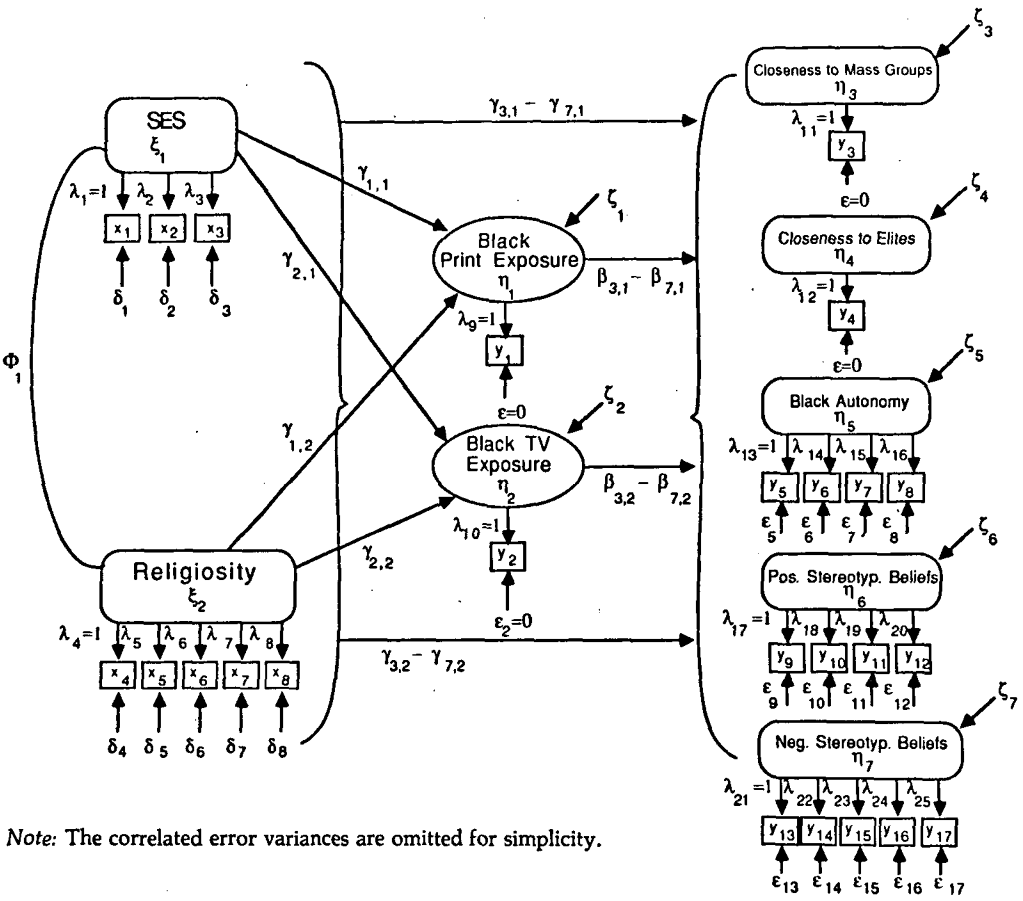
Measures

The two exogenous latent variables were measured by multiple indicators. The socioeconomic status construct was composed of the three indicators. Education was measured by an item that assessed the number of years of formal training the respondent received (X_1). Total family income was measured by an item that assessed the income, from all sources, of all persons living in that household (X_2). Occupation prestige was measured by an item that assessed the type of work in which the individual was engaged. The identified occupation was coded in terms of degree of prestige (X_3).

The religiosity construct was composed of five indicators expressed on a five-point scale. These items are (1) frequency of an individual's reading religious books (X_4), (2) frequency of an individual's listening to religious programs (X_5), (3) frequency of an individual's praying (X_6), (4) frequency of an individual's asking someone to pray for him or her (X_7), and (5) degree to which an individual considers him- or herself to be religious (X_8).

The two media exposure constructs were each composed of one indicator. The print exposure construct was represented by an item that asked the respondent's frequency of reading black literature (books,

Figure 1. Model of African-American Racial Belief System


SES
ξ_1
Religiosity
ξ_2
Black Print Exposure
η_1
Black TV Exposure
η_2
Closeness to Mass Groups
η_3
Closeness to Elites
η_4
Black Autonomy
η_5
Pos. Stereotyp. Beliefs
η_6
Neg. Stereotyp. Beliefs
η_7
Φ_1
$\gamma_{1,1}$
$\gamma_{2,1}$
$\gamma_{1,2}$
$\gamma_{2,2}$
$\gamma_{3,1} - \gamma_{7,1}$
$\gamma_{3,2} - \gamma_{7,2}$
$\beta_{3,1} - \beta_{7,1}$
$\beta_{3,2} - \beta_{7,2}$
ζ_1 ζ_2 ζ_3 ζ_4 ζ_5 ζ_6 ζ_7
$\lambda_1=1$ λ_2 λ_3
$\lambda_4=1$ λ_5 λ_6 λ_7 λ_8
$\lambda_9=1$ $\lambda_{10}=1$ $\lambda_{11}=1$ $\lambda_{12}=1$
$\lambda_{13}=1$ λ_{14} λ_{15} λ_{16}
$\lambda_{17}=1$ λ_{18} λ_{19} λ_{20}
$\lambda_{21}=1$ λ_{22} λ_{23} λ_{24} λ_{25}
x_1 x_2 x_3 x_4 x_5 x_6 x_7 x_8
δ_1 δ_2 δ_3 δ_4 δ_5 δ_6 δ_7 δ_8
y_1 y_2 y_3 y_4 y_5 y_6 y_7 y_8 y_9 y_{10} y_{11} y_{12} y_{13} y_{14} y_{15} y_{16} y_{17}
$\varepsilon=0$ $\varepsilon_2=0$
ε_5 ε_6 ε_7 ε_8 ε_9 ε_{10} ε_{11} ε_{12} ε_{13} ε_{14} ε_{15} ε_{16} ε_{17}

Note: The correlated error variances are omitted for simplicity.

magazines, and newspapers). This item was measured on a five-point scale (Y_1). The television exposure construct was represented by an item asking how often the respondent watched black television programs. This item was measured on a five-point scale (Y_2).

Five constructs are used to model the cognitive schemata that compose the black racial belief system—closeness to black mass groups, closeness to black elite groups, black autonomy, positive stereotypical beliefs of African-Americans, and negative stereotypical beliefs of African-Americans.

The construct of closeness to black mass groups was represented by one composite indicator (Y_3). This indicator was constructed from the sum of *yes* responses to items that asked individuals whether they felt close to poor blacks, religious and church-going blacks, young blacks, middle-class blacks, working-class blacks, and older blacks.

The construct of closeness to black elite groups was also represented by one composite indicator (Y_4). This indicator was constructed from responses to a five-point scale that asked individuals the degree to which they felt close to black elected officials, doctors, lawyers, and other black professionals.

The black autonomy construct contained four indicators, each expressed in a five-point agree-disagree format. These items were (1) Black children should study an African language (Y_5), (2) Blacks should always vote for black candidates when they run (Y_6), (3) Black people should shop in black-owned stores whenever possible (Y_7), and (4) Black parents should give their children African names (Y_8).

The construct of positive stereotypical beliefs of African-Americans contained four indicators, each measured on a four-point *very true-not true at all* format: (1) Blacks are hard working (Y_9), (2) Blacks help others (Y_{10}), (3) Blacks are honest (Y_{11}), and (4) Blacks are strong (Y_{12}).

Finally, the construct of negative stereotypical beliefs of African-Americans contained five indicators, each measured on a five-point *very true-not true at all* format: (1) Most blacks are lazy (Y_{13}), (2) Most blacks neglect their families (Y_{14}), (3) Most blacks are lying and trifling (Y_{15}), (4) Most blacks give up easily (Y_{16}), (5) Most blacks are weak (Y_{17}).

The Model

Figure 1 presents a diagram of the proposed structural model of black belief systems. SES and religiosity are the two exogenous latent constructs (ξ_1, ξ_2). The model shows that the two exogenous variables are posited to be correlated with each other and help to predict black print exposure (η_1), black television exposure (η_2), and each of the five belief system schema (η_3–η_7). Black print exposure (η_1) and black television exposure (η_2), in turn, also predict each of the belief system constructs.

Figure 1 follows the conventions of path analysis: theoretical constructs (latent variables) are drawn as circles, their respective measures are presented as squares, and causal relations are depicted as arrows. Parameters are indicated by Greek letters. Here, ξs (*ksis*) refer to the exogenous latent variables; ηs (*etas*) are endogenous latent variables. βs (*betas*) and γs (*gammas*) are the coefficients or direct causal effects linking the latent variables. The ζs (*zetas*) indicate errors in the equations. Φ (phi) is the covariance matrix of the exogenous latent variables.[8] By correlating these error variances, we assumed that the variances left unexplained in these constructs are due to common antecedents. The λs (*lambdas*) are the factor loadings or the coefficients of the indicators regressed on the unobserved dimension. To establish the unit of measurement for each factor, the factor loading of one of its indicators is constrained to 1. Finally, the εs (*epsilons*) denote measurement errors associated with the y observable indicators; the δs (*deltas*) denote measurement errors associated with the x observable indicators.

To estimate parameters and test hypothes, the structural equation methodology, LISREL, developed by Joreskog and Sorbom (1984), was used. There are many reasons to use structural equation modeling (see Bollen n.d.; Goldberger 1973; Maruyama and McGarvey 1980). We chose it because (1) our observed variables contain error measurement, (2) we assumed interdependence or simultaneous causation among our outcome variables, and (3) we assumed that certain explanatory variables may not have been observed. LISREL, one of the available models for handling systems of structural equations, was able to accommodate these considerations quite easily. This procedure analyzes systems of relations among variables in which error measurement and errors in equations are modeled explicitly. Parameters are maximum likelihood estimates, and an overall chi-squared goodness-of-fit measure is provided for any model tested.[9] The correlation matrix was used to estimate the parameters.[10]

LISREL models have two components. The first is the measurement model in which the latent theoretical constructs, their observed indicators, and errors in measurement of those indicators are specified. The second is the causal model where causal relationships between the latent variables are specified.

Table 1. Measurement Coefficients for African-American Racial Belief System

Construct	Unstandardized λ^*	Standardized λ	Errors
Socioeconomic status (ξ_1)			
X_1	1.00[a]	.84	.30[b]
X_2	.72	.60	.64
X_3	.62	.52	.73
Religiosity (ξ_2)			
X_4	1.00[a]	.68	.55
X_5	.87	.58	.66
X_6	1.00	.67	.55
X_7	.86	.58	.67
X_8	.97	.65	.58
Black print media (η_1)			
Y_1	1.00[a]	1.00	.00[c,d]
Exposure to black TV (η_2)			
Y_2	1.00[a]	1.00	.00[c]
Closeness to black masses (η_3)			
Y_3	1.00[a]	1.00	.00[c]
Closeness to black elites (η_4)			
Y_4	1.00[a]	1.00	.00[c]
Black autonomy (η_5)			
Y_5	1.00[a]	.46	.79
Y_6	1.12	.51	.74
Y_7	1.06	.48	.76
Y_8	1.00	.45	.79
Positive stereotypes (η_6)			
Y_9	1.00[a]	.57	.68
Y_{10}	1.09	.62	.62
Y_{11}	1.10	.57	.67
Y_{12}	1.01	.57	.67
Negative stereotypes (η_7)			
Y_{13}	1.00[a]	.72	.48
Y_{14}	.96	.69	.53
Y_{15}	1.05	.75	.43
Y_{16}	.80	.58	.67
Y_{17}	.92	.66	.57

Note: Number of cases = 865.

[a] Coefficient fixed at 1.00.

[b] This and following coefficients θ_δ.

[c] Error fixed at .0.

[d] This and following coefficients θ_ϵ.

*$p \leq .0005$ for all free coefficients.

Results

Measurement Model

We begin our examination of the component fit model measures by inspecting the measurement parameters presented in Tables 1–3, namely, λy, λx, ψ, $\theta\epsilon$ and θ_{δ}. The standardized factor loadings for the three indicators of the socioeconomic construct ranged from .52 to .84, and these values were all statistically significant.[11] The factor loadings for the five indicators of the religiosity construct ranged from .58 to .67 and, again, all these values are statistically significant.

For the y indicators of black autonomy, the factor loadings ranged from .45 to .51. For the y indicators of negative stereotypical beliefs of African-Americans, the factor loadings ranged from .58 to .72. For the y indicators of positive stereotypical beliefs, the factor loadings ranged from .57 to .62. All the y indicators were adequate reflections of their respective constructs and were statistically significant. Since the constructs of black print exposure, black television exposure, closeness to mass groups and closeness to elite groups are each composed of one indicator and thus fixed to a factor loading of 1, no measure of statistical significance is possible.[12]

Table 2. Phi Matrix (Standardized Solution)[a]

	Socioeconomic Status	Religiosity
Socioeconomic status	1.00	—
Religiosity	–.29	1.00

[a] $p \leq .0005$ for all free coefficients.

Table 3. PSI Matrix (Variance-Covariance Matrix of Correlated ζs)

Variables	Black Print	Black TV	Black Autonomy	Positive Stereotype	Negative Stereotype	Closeness to Masses	Closeness to Elite
Black print	.90*[a] (.90)[b]	—	—	—	—	—	—
Black TV	.38* (.38)	1.00* (1.00)	—	—	—	—	—
Black autonomy	.0[c]	.0[c]	.19* (.93)	—	—	—	—
Positive stereotype	.0[c]	.0[c]	.14* (.14)	.30* (.95)	—	—	—
Negative stereotype	.0[c]	.0[c]	–.00 (–.00)	–.13* (–.33)	.48* (.92)	—	—
Closeness to black masses	.0[c]	.0[c]	.07* (.15)	.11* (.19)	–.02 (–.02)	.75* (.75)	—
Closeness to black elites	.0[c]	.0[c]	.09* (.20)	.08* (.14)	.02 (.03)	.35* (.35)	.81* (.81)

[a] Variance-covariance coefficient.

[b] Correlation coefficient.

[c] Fixed at 0.

* $p \leq .01$.

Table 4. Unstandardized and Standardized Regression Coefficients for African-American Racial Belief System

Independent Variables	Black Print	Black TV	Closeness to Masses	Closeness to Elite	Black Autonomy	Positive Stereotype	Negative Stereotype
ξ_1 Socioeconomic status	.38*[a]	-.06	-.15*	-.27*	-.09*	-.02	-.19*
	(.32)[b]	(-.05)	(-.13)	(-.23)	(-.17)	(-.03)	(-.22)
ξ_2 Religiosity	.13*	.03	.63*	.44*	.01	.12*	.08**
	(.09)	(.02)	(.42)	(.20)	(.01)	(.15)	(.08)
η_1 Black print	—	—	.04	.02	.01	.01	-.06**
			(.04)	(.02)	(.02)	(.01)	(-.08)
η_2 Black TV	—	—	.13*	.11*	.09*	.08*	.03
			(.13)	(.11)	(.19)	(.15)	(.04)
R^2	.10	.00	.25	.19	.07	.05	.08

Note: $n = 865$; total coefficient of determination = .41; goodness-of-fit index = .95; adjusted goodness-of-fit index = .93; critical $N = 430$; $\chi^2 = 566.27$; $df = 243$; $\chi^2/df = 2.33$.

[a] Unstandardized coefficient.

[b] Standardized coefficient.

*$p \leq .01$.

**$p \leq .10$.

Eight of the eleven correlated error variances reached statistical significance, with varying levels of magnitude. The statistically significant relationships among the correlated error variances indicates that those constructs have common, yet unknown, antecedent causes. Moderately large coefficients were obtained for the relationships between black-oriented television exposure and black print exposure (.38), closeness to the black masses and closeness to black elites (.35) and negative stereotypical belief and positive stereotypical beliefs (−.33). The other relationships were fairly small; the next highest was the relationship between closeness to black elites and black autonomy (.20).[13]

The relationships that did not reach statistical significance were (1) negative stereotypical beliefs and closeness to black masses, (2) negative stereotypical beliefs and closeness to black elite groups, and (3) negative stereotypical beliefs and black autonomy. It is worth noting that the construct of negative stereotypical beliefs was an element in all these statistically nonsignificant relationships; that is, whatever the causes for negative stereotypical beliefs, they do not appear to be the same for feelings of closeness to mass and elite groups or for expressing a greater sense of black autonomy.

Substantive Findings

Table 4 contains the parameters estimated as part of the causal model. Hypothesis 1 and 2 posit that those better placed in the social structure are more likely to attend to the black print media and less likely to attend to black television programming. As predicted, the results indicated a fairly strong, statistically significant, positive relationship between SES and black print media exposure. While SES had a negative influence on black television exposure as predicted, this relationship was small and statistically nonsignificant. We predicted also a positive relationship between religiosity and black print exposure, and between religiosity and black television exposure (hypotheses 3 and 4). Their relationship between religiosity and black print exposure received support, but no statistical significance was found for the relationship of religiosity and black television exposure.

Turning to the relationship of SES to various black belief structures (hypotheses 5–9), four of the five relationships were statistically significant and in the hypothesized direction. The magnitude of the effects varied according to schema. Those better placed in the social structure expressed less of a sense of black autonomy ($\beta = -.09$, *beta* = −.17), felt less of a closeness with mass groups ($\beta = -.15$, *beta* = −.13) or elite groups ($\beta = -.27$, *beta* = −.23), and were less likely to endorse the negative stereotypical beliefs about blacks ($\beta = -.19$), *beta* = −.22). The relationship between socioeconomic status and positive stereotypical beliefs was not statistically significant.

With respect to religiosity and black belief structures (hypotheses 10–14) the results indicated that as predicted, those scoring high on the religiosity construct are more likely to embrace black autonomy, to express a closeness to both mass and elite groups in the black community, and to endorse both negative and positive stereotypical beliefs of blacks as a group. All the above relationships were statistically significant, except for the relationship between religiosity and black autonomy, but they varied considerably in magnitude. For example, the coefficient for closeness to black mass groups was fairly high ($\beta = .63$, *beta* = .42), but the coefficient for negative stereotypes was quite small ($\beta = .08$, *beta* = .08).

The final set of hypotheses (15–24) refers to the relationship of black print media and television exposure to the racial belief system. First we suggested that there would be a negative relationship between black print exposure and the endorsement of negative stereotypical beliefs. This relationship received support. On the other hand, black print exposure was assumed to encourage positive stereotypical beliefs, closeness to both black elite and black mass groups, and black autonomy. While in the predicted direction, none of these relationships were statistically significant.

Finally, we argued for a positive relationship of black television exposure to black autonomy, positive stereotypical beliefs, and closeness to both mass and elite groups but a negative relationship between black television exposure and negative stereotypical beliefs. Evidence for four of these five relationships was forthcoming. The only exception was for the hypothesized relationship between television exposure and negative stereotypical beliefs about African-Americans, which did not reach statistical significance.

A rough measure of the horizontal constraint found between the various schemata that comprise the belief system can be generated by analyzing the correlations between the five belief system constructs (see Table 5).[14] Every schema of the belief system is at least moderately linked horizontally to the other schemata. On inspection, there was the expected correlation (.56) between closeness to the black masses and closeness to black elites. Further, we found the expected negative correlation (which was moderate at −.30) between negative stereotypical and positive stereotypical beliefs. The closeness schemata and the stereotype schemata formed two clusters within the belief system. There was a strong link between the two closeness schemata. The link between the stereotype beliefs are linked to the belief system only through positive stereotypical beliefs. That positive and negative stereotypes represent two separate schemata is illustrated both by the different relationships between them and the rest of the constructs and by the different strength of associations of each with the three other belief system constructs. We saw links between the two clusters in pos-

Table 5. Correlations between Racial Belief System Schemata

Variables	Black Autonomy	Closeness to Black Masses	Closeness to Black Elites	Positive Stereotype	Negative Stereotype
Full sample					
Black autonomy	1.00	—	—	—	—
Closeness to black masses	.22	1.00	—	—	—
Closeness to black elites	.27	.56	1.00	—	—
Positive stereotype	.18	.29	.23	1.00	—
Negative stereotype	.04	.07	.13	−.30	1.00
High education blacks					
Black autonomy	1.00	—	—	—	—
Closeness to black masses	.20	1.00	—	—	—
Closeness to black elites	.10	.39	1.00	—	—
Positive stereotype	.37	.25	.06	1.00	—
Negative stereotype	−.27	−.03	−.02	−.54	1.00
Blacks with less education					
Black autonomy	1.00	—	—	—	—
Closeness to black masses	.33	1.00	—	—	—
Closeness to black elites	.42	.60	1.00	—	—
Positive stereotypes	.29	.30	.28	1.00	—
Negative stereotypes	−.09	.05	.11	−.23	1.00

itive stereotypical beliefs' relation with closeness to the masses (.29) and closeness to elites (.23). The black autonomy schema was positively correlated to positive stereotypical beliefs and to both schemata of the closeness cluster.

We now will assess the overall fit of the model (see Table 4). A number of criteria may be employed to evaluate the goodness of fit of any model. Given that the likelihood ratio test is very sensitive to sample size, almost no theoretical structure will adequately account for the observed covariances or correlations for large samples (Bentler and Bonnet 1980). With a sample size similar to ours, Wheaton and his colleagues (1977) used a relative chi-squared and its degrees of freedom, to assess the goodness of fit. This index ostensibly takes sample size into account, and a ratio of three or less has been considered quite adequate (see Carmines and McIver 1981). Applying this measure of fit, our model exhibits a good fit with a relative chi-squared of 2.33.[15]

Still another measure of the overall fit of a model is the total coefficient of determination (TCD). The TCD demonstrates how well the structural equations (i.e., hypothetical theoretical relations) account for the number of variables. Although more experience is needed to ascertain when the TCD is large or small, the .407 estimate obtained for this model seems acceptable.

Thus, the overall measures of fit suggest an adequate model. Similarly, the component measures of fit yielded, for the most part, impressive estimates. Starting with the measurement parameters, the *lambdas*, or factor loadings, were moderate-to-high in value. Moreover, the correlated error variances for the media constructs and for eight of the eleven belief constructs as predicted were all statistically significant. The magnitude of these effects was moderate. The construct of negative stereotypical beliefs of African-Americans was included in all these statistically nonsignificant relationships.

Finally, the hypothesized theoretical relationships received reasonable support. While most of the hypothesized relationships were supported statistically, the magnitude of some of these effects was fairly small. It should be noted that black print media exposure had a statistically significant relationship with only one of the five belief constructs whereas television exposure had a relationship with four of them. As both media constructs were composed of only one indicator, their nonsignificant relationships with the belief constructs may be due either to the lack of a true relationship or to random measurement error. Without any assessment of error in these measures, it was not possible to make the distinction. However, a statistically significant relationship between these one-indicator media constructs and the belief constructs does suggest that these relationships actually exist (Bagozzi and Yi 1987).

Discussion

Our model has been shown to be relatively powerful and parsimonious. There exists a well-defined black racial belief system that structures African-Americans' cognitive processing of information and is influenced by individuals' status in society and guidance by religious beliefs. There are sensible and moderate linkages among all of the schemata except negative stereotypical beliefs. However, this latter construct is linked to positive stereotypical beliefs. Moreover the schemata are moderately-to-strongly linked along a horizontal dimension. We are confident that a moderately loose African-American racial belief system exists and that it helps structure information about political and social reality.

Additionally, one's place in the social structure and the centrality of religious activity in one's life have powerful and theoretically well understood effects on the racial belief system. The media play an intermediate and important role in further shaping the content of this belief system.

Higher placement in the social structure has negative effects on black autonomy, closeness to the black masses, and closeness to black elites. All these relationships suggest that as blacks move up the social structure and become more affluent, they are less likely to adopt a view of black identity centered on separation from white society. At the same time they are affectively more remote from the black community, including their own status peers, as Jackman and Jackman (1983) also found. The negative relationship between socioeconomic status and negative stereotypical beliefs suggest that while those of higher status feel more distant from black people, they also are not as negative toward the black community as are lower status respondents. These findings do not directly speak to Wilson's thesis that social stratification has increased *over time* or to the thesis that the *political* agendas of different strata of African-Americans are different due to conflicting class interests. However, they do tend to confirm that African-Americans of different strata have different psychological orientations toward racial identity. These findings highlight the importance of further testing the effect of social stratification on the psychological orientation, political preferences, and social behavior of different classes of African-Americans.

We used this opportunity to assess the levels of constraint between the racial belief system schemata. Table 5 shows the correlations between the schemata for the full sample, blacks with more education, and blacks with less education. The stratification point was between African-Americans with one year of college or more and those with less than a full year of college. While it is apparent from Table

5 that the correlation table for African-Americans with less education follows the pattern for the full model, it is clear that there are some interesting distinctions between the relationships between the schemata for blacks with different levels of education. Blacks with less education (a reasonable proxy for African-Americans for relative status) feel closer to both elite blacks and the masses of blacks than do blacks with more education. For blacks with higher levels of education the black autonomy construct was tied more closely to the positive and negative stereotypical beliefs constructs.[16]

Social status also helps to condition the route by which blacks adopt the political aspect of the African-American racial belief system. For those of less status, the black autonomy construct is associated with how close they feel to black people as a whole: integration into the black community goes hand in hand with a political dimension to racial consciousness. For those with higher status, the political correlation is not with feeling close to the black community but with positive image of the race: the accepting of positive and rejecting of negative stereotypical beliefs. These results suggest the need for further research on the class and status aspects of racial consciousness and on its relationship to political behavior.

The importance of status differences was also found in the negative relationship between socioeconomic status and religiosity. As we expected, those with higher status are less involved in religious activity. Religiosity has a direct and positive relationship with exposure to black media, positive stereotypical beliefs, and closeness to both the mass of blacks and elite blacks. These findings all suggest that respondents with a highly developed degree of religiosity, other things being equal, feel a strong positive relationship to the black community.[17] The positive association between religiosity and negative stereotypical beliefs of African-Americans also suggests that this positive relationship to the black community is not an uncritical one.

The fact that religiosity did not predict a sense of group autonomy points to the complexity of black religiosity alluded to earlier. Indeed, the message of spiritual, social, and political liberation from bondage may be strongest in blacks who have been socialized in religious denominations that encourage a sense of racial solidarity. In essence, we think that the communal aspect of black religiosity will manifest itself mostly in black churches that preach as part of their gospel message, social liberation based on racial group membership. This notion is closely linked to the thesis of Hunt and Hunt (1977) that nonsectarian black churches have the strongest impact on the crystalization of racial consciousness. It would be revealing to tease out the influence of church type on black autonomy.

Exposure to the black media was also an important predictor of the content of black belief schemata. Overall, the media's position as intermediate interpreters of reality between an individual and his or her environment was generally confirmed. As in most studies that include the media as predictors of beliefs, television was found to have the most dominant influence. While neither black print media nor black television had statistically significant effects on negative stereotypical beliefs of African-Americans, television had statistically significant effects on other racial beliefs. Specifically, it had a positive effect on the identification of positive stereotypical beliefs, the strength of the black autonomy schema, and closeness to the masses of blacks. Interestingly, it had a negative effect on the closeness to elite elements in the black community.

As intimated above, black print media were related to only one of the five racial belief constructs. Part of this puzzling finding may be due to the wide variety of black print media, for example, newspapers, magazines, and books. Because these different media extend across a very broad ideological spectrum and cater to different segments of the black community, it is very difficult to assume with any degree of certainty what type of content is involved in overall exposure to "black print media." A measure or measures showing which print medium and which type of content within that medium reach the individual would provide useful information.

Also, it would be desirable to have multiple measures of black television content categories. One such category could include program types showing blacks in various dramatic or entertainment roles. The other category could include program types of a more informational nature, for example, black public affairs shows and talk shows. Within each of these categories, different measures could show how much attention and time is given to the various program types. Similarly, the amount of attention and amount of time given to each print medium could be assessed. The sum of the influence of SES, religiosity, and the media tend to conform theses advanced by Yancey and Taylor that the political and social environment play an important role in the development of ethnic and racial consciousness (Taylor 1979; Yancey, Ericksen, and Juliani 1976).

It is clear that the next step is to gain a greater understanding of the influence of the African-American racial belief system on political and social behavior. Preliminary analyses shows that the belief system has a strong effect on political participation. Results from this set of analyses will be reported in the future.

We also need to explore how this belief system is structured across different strata, genders, and cohorts of African-Americans. Such critical hypotheses as Wilson's thesis that race is declining in significance in relation to class as a determiner of African-American life chances will be tested in the next stage of this project. Wilson has argued—and Freeman has to an extent shown—that younger cohorts of African-Americans are economically integrated into U.S. society (Freeman 1976; Wilson 1980). Kilson, among others, has argued that this new more-economically-integrated black middle class will start to diverge politically from the rest of the black community (Kilson 1983). Similarly, if class is becoming more of a determinant of one's place in society, the cognitive heuristic represented by the racial belief system would be less useful in processing information and we should see the African-American racial belief system begin to weaken. The heuristic of substituting group utility for individual utility should be utilized by African-Americans only so long as they perceive their life chances as substantially determined by their race. Since we emphasize the role of perceptions, or the subjective status of race to individuals, the strength of a racial belief system for African-Americans could be determined by the political and ideological debate on the status of race in the United States, as well as by objective factors. While it would be difficult to capture the dynamic properties of the interaction between class and race in cross-sectional analysis, we can use cohort analysis to test whether different structures and levels of the racial identity belief system pertain to different cohorts and strata of African-Americans.

Future research might explore not only the effect of black-oriented television and print media on black racial beliefs, but also the effect of these media when content of the majority media is included as well. Finally, although our proposed model was supported empirically, structural variations of this conceptualization should be explored across such different subgroups as gender, age, and region. Not only would such attempts extend the representativeness of the proposed model, but they also may reveal interaction effects. Only when we understand the differences in the interpretation and understanding of the social world by black and white citizens will we be able to facilitate the type of discourse that will serve to narrow, rather than widen, the racial gulf in this society.

Notes

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1. Kathy Hulik, "Race Differences Persist, Study Says," *Ann Arbor News*, 15 December 1986, p. A3.

2. Claudia H. Deutsch, "Still on the Outside Looking In," *New York Times*, 5 July 1987, sec. 3, p. 1; Isabel Wilkerson, "Infant Mortality: Frightful Odds in Inner City," *New York Times*, 26 June 1987, nat. ed., p. 1; Lena Williams, "For Black Professional, the Obstacles Remain," *New York Times*, 14 July 1987, nat. ed., p. 10.

3. For two exceptions, see Allen and Hatchett 1986, and Schuman, Steeh, and Bobo 1985.

4. The hypotheses are numbered to correspond to Figure 1.

5. We would have liked to investigate the affect of the major media outlets on the African-American racial belief system, but unfortunately, our data set did not contain the requisite questions.

6. Some of the television material presented by blacks or about blacks takes the form of cultural or public affairs programming. Frequently, these programs are produced or written by blacks and generally are more positive and explicit in their black group orientations. Here, too, this content type is referred to as "black-oriented" or, more typically, "black television."

7. The sample was drawn according to a multistage area probability procedure designed so that every black household had an equal probability of being selected for the study. The sample was based on the 1970 census distribution of the black population; 76 primary areas were selected and stratified by racial composition (number of black households) and region. Then smaller geographical areas (sample places and clusters) were randomly chosen from these strata. Household sampling and interviewing were conducted in these smaller geographical areas, usually representing city blocks or groups of blocks.

Within each selected black household, one interviewee was randomly chosen from among those eligible for the study. Eligibility was restricted to persons 18 and older, self-identified as African-Americans and U.S. citizens.

The final sample consisted of 2,107 completed interviews; the response rate was 67%. This national cross-section sample was fairly representative of the black population as reported in the 1980 census. If respondents had living family members from at least two other generations, we attempted to interview one randomly selected representative from each of those two generations. Cross-section respondents were reinterviewed with a form of the three-generation instrument. Of the 2,107 respondent in the cross-section study, 1,122, or roughly 52%, were eligible for the three-generation study. Reinterviews were obtained from 865 of these respondents, a 77% response rate. This study is based on data from the reinterview study because this subsample survey contained the variables that were of interest.

8. Although not shown in Figure 1, the error variances were correlated within the belief constructs and within the media constructs. These correlations are denoted by ψs (*psis*).

9. Although not entirely conclusive, we used empirical means to test for identification. We used different starting values with the correlation matrix and obtained the same results. Finally, the model was estimated with the LISREL VI statistical package, which also performs tests for model identification.

10. Because the units of measurement of the observed variables are arbitrary, we used a correlation matrix in the analysis. Copies of the correlation matrix with standard deviations and means used to estimate the model are available from us on request.

11. Several assumptions are implied when LISREL is used. First, the errors in the equations (ζs) are assumed to be uncorrelated with the exogenous latent variables (ξs). Second, the measurement errors (δs,ϵs) are assumed to be independent from ξs, ηs, λ_xs and λ_ys. Third, measurement errors are further assumed to be uncorrelated among themselves. Finally, since we are using a maximum likelihood estimator, the observed indicators (*Y*s and *X*s) are assumed to be distributed multinormally.

12. All the measurement errors for the X and the Y indicators (δs and ϵs) were statistically significant. For the constructs with only one measure, the measurement errors were fixed to 0.

13. These constructs were specified to have correlated error variances because they are subdimensions of larger global constructs. Thus, one would expect their error to be correlated. For example, the two media constructs were considered to be subdimensions of the construct black media exposure. Each of the five belief system constructs were considered to be subdimensions of an African-American racial belief system. For an excellent discussion of the importance of explicitly considering assumptions

about correlated error variances, see Hargens 1988.

14. Since we are *not* specifying causal relations between the *belief* schemata (differently from the other latent constructs), it is appropriate to inspect the correlations and not causal paramaters. We claim only that these constructs are bounded horizontally.

15. The sensitivity of the χ^2/df measure to sample size has led to the suggestion that other fit measures be used (Bollen n.d.). One possibility is the goodness-of-fit index (GFI). It indicates the relative amount of variance explained by a model. Its values range in most cases from 0 to 1. The GFI for our model is .95, which suggests a good fit. Since GFI does not adjust for degrees of freedom, we used a modified version of it called the adjusted goodness-of-fit index (AGFI), developed by Joreskog and Sorbom (1984). This index is independent of sample size and relatively robust against departures from normality. Again, AGFI will generally range from 0 to 1, and values equal to or greater than about .90 suggest a meaningful model from a pragmatic point of view (Bagozzi and Yi 1987). The AGFI for our model is .93. The final criterion we used for evaluating a model's global fit is the critical N (CN) index (Hoelter 1983). This estimate permits the assessment of a model's fit relative to an identically hypothesized model estimated with different sample sizes. Hoelter suggests that a CN greater than 200 for a single sample might be a "tentative" rule of thumb for deciding whether the discrepancy between an observed and implied variance-covariance matrix is trivial or not. Our CN value was 430.

16. A chi-squared difference test showed that the model fits the two strata better when the values between higher- and less-educated African-Americans are allowed to differ from each other (for more information on this test see Hayduk 1987).

17. We ran a simple regression with religiosity as the dependent variable and SES and the degree of black participation in the respondent's church as the predictors. SES had the expected negative association with religiosity, while the degree of black church membership was a positive, significant predictor of religiosity. This finding suggests that religiosity for African-Americans is related to the black church as a critical institution in forming religious beliefs.

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LIFE SPAN CIVIC DEVELOPMENT AND VOTING PARTICIPATION

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Although a variety of age-related processes are known to affect rates of political participation over the adult life span, little is known about their interrelationships and relative impact. We set out a theory of life span civic development that focuses on how age-related changes in community attachment, strength of partisanship, church attendance, government responsiveness, family income, and civic competence impinge on voting participation. To test the theory, we estimated the coefficients of a structural equation model using data from nine National Election Studies combined into a large, cross-sectional time series data set. The model specifies the age-related processes and also controls for the effects of a large number of other variables. Overall, about one-half of the age-related increases in voting participation were attributable to these processes.

We posit a theory of civic development that focuses on the nature and causes of age-related changes in levels of political participation. Because of these changes, participation among older persons is higher than would otherwise be expected. Although we believe this would apply to most conventional modes of political participation (Elder et al. 1985), we focus on voting participation. We test the theory through a secondary analysis of survey data from nine presidential election surveys of the U.S. National Election Studies (NES).[1]

Aging-related Processes

Analysts employing a variety of disciplinary perspectives have found that age is a useful explanatory tool. As the psychologists Birren and Cunningham (1985) point out, however, "As we approach a real understanding of why age groups differ, we will no longer use age as a variable. Presumably differences in behavior with age are associated with environmental causes, genetic causes, and individual decision-making processes" (pp. 12–13).

Social scientists who have studied the causes underlying individual-level variations in rates and modes of political participation have typically used the variable of chronological age as a surrogate for various changes in the life cycle thought to affect motives, opportunities, and resources relevant to political participation. Among young adults, rates of political participation tend to be low. This is likely due to the primacy of such nonpolitical concerns as obtaining an education, finding a mate, and establishing a career. Young adults are also inexperienced politically and often lack the skills and the knowledge about politics that makes political participation both easier and more meaningful.

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Among middle-aged persons increased church attendance, stronger community attachments, greater participation in organizations, and growing family income tend to enhance rates of political participation, as does the tendency for political party attachments to become stronger with time. As adults grow older, they also acquire political experience.

Among older persons, declining family income, poorer health and increasing disability undoubtedly lead to a drop in rates of political participation.[2] It has been difficult to isolate the effects of these changes on political participation, however, because of the confounding effects of other variables such as education, gender, and marital status. Older cohorts have less education than younger ones. The ratio of women to men increases with age, and women traditionally have participated in politics less than men. In addition, a disproportionate number of the elderly, especially widows, live alone. Because of this, the motive to participate in politics that might be supplied by a politically active spouse is lacking.

Many analysts have noted the effects of aging processes on political participation. A number of studies have noted the various positive relationships that exist between age, political interest, and political participation (Campbell 1971; Crittenden 1963; Curtis and Lambert 1976; Glenn and Grimes 1968; Verba and Nie 1972). In a recent study, Neuman (1986) looked at the linkage of political sophistication to age-related increases in political participation and argued that political sophistication was the product of three stages of socialization: early, adolescent, and adult. In early socialization, the family is important; in the adolescent stage, formal schooling and sex role socialization; and in the adult phase, group participation, income, and age. Neuman, however, provides only a limited discussion of the aging process, noting that "age itself, or simply growing older, allows for a gradual political learning" and that "a lessening of the competing demands on time from family rearing and career development in early adulthood may also lead to increased political involvement among older citizens" (p. 114). The effects of age on political participation warrant a more comprehensive and detailed analysis. The rate of political learning, rather than being constant over the adult life span, is likely to vary and be marginally greater for young persons than for the middle-aged and elderly. Although family, child rearing and career development place time demands on younger adults that may lower their level of political participation, these age-related processes are associated with activities (e.g., involvement in community and religious institutions) that also incline people toward increased political participation. There are also age-related processes in later life, such as declines in family income, poorer health, and increasing disability, that can cause decreases in political participation.

To understand more fully the effects of various age-related changes on political participation we need to disentangle them from each other and from the effects of other variables. A key problem is that no data sets have measures for all of the possible age-related intervening variables that may have an effect on political participation, including voting. Accordingly, we focus here on age-related variables for which there are measures in the NES data sets: civic competence, community attachment, church attendance, the strength of political party attachments, government responsiveness, and family income. There are other age-related intervening variables that we believe to be important, such as health, disability, group and organizational involvement, and citizen duty. Without direct measures of these over time, however, we must rely on age as a surrogate for them in this analysis. To the extent that the variables for which we do have measures are able to account for age-related changes in voting participation, the analysis will serve to clarify processes of civic development and reduce the unidentified residual effects embedded in the variable of chronological age.

Data and Methods

To test our civic development theory we conducted secondary analysis of survey data from nine presidential election cross-sectional surveys of the U.S. National Election Studies dating from 1952 through 1984, a period of 32 years.[3] In two of the years—1960 and 1976—cases were weighted, due to disproportionate sampling; however, except for the estimation of population marginals, we work here with the unweighted data.

To test the theory we pooled data from all nine presidential election studies into a large cross-sectional time series data file containing 16,794 cases. We conducted path analysis to estimate the coefficients of a specific structural equation model that included 26 exogenous and 7 endogenous variables. Appendix A details the measures that we used for these variables.

Among the exogenous variables are age and age-squared, which we use to estimate the linear and nonlinear effects of various age-related processes on political participation.[4] Other exogenous variables include standard demographic items, variables to represent interaction effects, a variable to represent the Vietnam cohort, and variables to represent specific trends and period effects.[5] We chose these because of their presumed effects on one or more of the endogenous variables based either on our own hypotheses or on previous research findings. We must include them within the model in order to control for their effects and thereby obtain unbiased estimates of the effects of the age-related processes on political participation. The rationale for the inclusion of each of the control variables is presented in Appendix B.

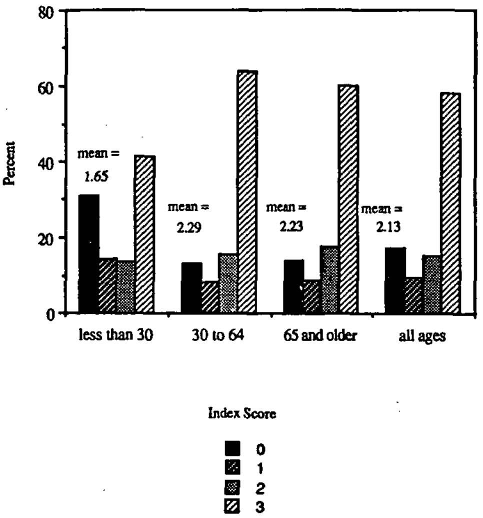
The method of pooling cross-sectional data raises a number of complex methodological issues. One issue is conceptual equivalence. For a measure to be valid the meaning of a survey question to respondents (and its response categories) should remain the same or much the same across time. For the exogenous variables in this study, which are mostly demographic items, this is not a problem. For the endogenous variables, we have either used the same survey questions over time to construct an index or conducted factor analysis to assess the stability of construct-indicator linkages over time (see Appendix C). Another issue in the analysis of data over time—autocorrelation—is not a serious problem with these pooled data because the same units (individuals) are not being observed over time (Stimson 1985).

Our method has an important advantage over such other methods as the analysis of simple cohort tables. With pooled data we can estimate the effects of aging on political participation while simultaneously controlling for specific cohort effects, period effects, and the effects of such respondent characteristics as education, income, and occupation.[6]

Age Groups and Voting Participation

We define voting participation, our dependent variable, as involvement in activities linked directly to choosing between candidates for public office. Our measure is a simple index varying from zero to three, reflecting how many activities the respondent engaged in: registering to vote, voting in the presidential election four years before, and voting in the most recent presidential election (see Appendix A).

Figure 1. Voting Participation by Age Group, All Years



There are likely to be errors for measures of all of these activities due to misreporting. It is well known that more people report having voted than actually vote (e.g., Traugott and Katosh 1979; Katosh and Traugott 1981). Fortunately, for purposes of analysis, "vote misreporters resemble actual voters more closely than they do non-voters, so adding them to the ranks of voters does not introduce major compositional changes"; thus, "our understanding of the factors that influence voting appears to be largely unaffected by the misreporting phenomenon" (Sigelman 1982; see also Anderson and Silver 1986).

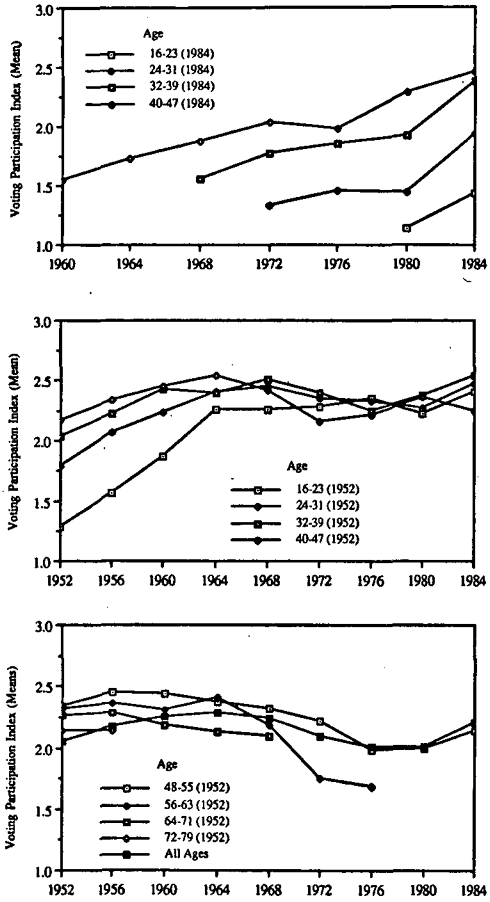
An exception to this generalization is age. Misreporting is highest among the young, where there is greatest discrepancy between sense of citizen duty and actual rates of political participation. Greater misreporting by the young would attenuate any positive correlation between age and voting turnout, so that the true relationship would actually be stronger than that observed.

Figure 1 shows the distribution of the voting participation index by age group (less than 30, 30–64, and 65 and older) for the entire pooled data set. Even with misreporting—and consistent with previous studies (e.g., Nie, Verba, and Kim 1974; Verba and Nie 1972)—voting participation was lowest among the young, highest among the middle-age group, and intermediate among the elderly.

Figure 2 is comprised of three graphs that show mean values on the voting participation index for different cohorts. The first graph shows the four youngest cohorts appearing in the pooled data set. Within each of these cohorts, there are notable gains in the voting participation index over the first few elections in which their members are eligible to vote. The second graph shows the four cohorts preceding the youngest cohorts. It shows clearly the convergence of levels of voting participation of cohorts as they age. For example, the youngest cohort in this group in 1952 (age 16–23) has converged to the level of the other cohorts by 1972, when they were 36–43. The third graph shows the four oldest cohorts appearing in the cross-sectional time series file, as well as the overall mean for all age groups. There is evidence here of a decline in voting participation with advancing age, due most probably to declines in family income, poorer health, and increasing disability.

It is apparent that changes in voting participation of cohorts over the 32-year period are nonlinear. This supports strongly the hypothesis that age-related variables are important causes of differences in voting participation between age groups. Several other hypotheses are not supported. The observed pattern of changes in voting participation within cohorts over time is unlikely to be due to period effects because the changes occurred without regard to any trend in the overall mean (cf. Hout and Knoke 1975; Reiter 1979). Furthermore, differences in voting participation between age groups are unlikely to be due to cohort effects because the differences do not persist—the voting participation of cohorts converges in middle age (see Glenn 1977).

Figure 2. Voting Participation by Cohort



The Model

Our theory of life span civic development assumes that political participation in its various forms is social behavior based on accepted knowledge and dispositions, shaped and maintained by a variety of material, purposive, and social rewards. It therefore suggests a broader range of incentives for political participation than those implied by rational choice theory as posited by, for example, Goodin and Dryzek (1980). It accepts these authors' notion that participation is influenced by contextual factors but not the notion that participation is necessarily and exclusively instrumental, as opposed to expressive or symbolic, in character.

Figure 3 illustrates our theory of civic development in the form of an arrow diagram. To highlight the effects of aging the diagram shows only those paths originating from the exogenous variables age and age-squared and those between the endogenous variables; it omits paths originating from other exogenous variables. To test the theory we estimated the coefficients of a specific model that included seven equations (one equation for each endogenous variable). We assume that the model is recursive and that correlations between the errors of the endogenous variables are zero, enabling us to use ordinary least squares to estimate the coefficients of the model. (The largest observed correlation between actual residuals, saved after conducting regression analysis on each equation, was .07. Most were close to .00.) Table 1 shows the unstandardized structural coefficients (*b*s), along with their standard errors, and the standardized path coefficients (beta weights).

In Figure 3 the direct paths to voting participation represent the effects of age-related changes for which we do not have measures (e.g., citizen duty, health, disability). The indirect paths to voting participation are through the intervening variables of civic competence, community attachment, church attendance, strength of partisanship, government responsiveness, and family income. The following details

Table 1. Regression Coefficients in Model of Voting Participation

Variable	*b*	s_b	Beta Weight
Community Attachment[a]			
Constant	-1.287	.026	—
Family income	.003	9.77E-04[b]	.02
Household size	.030	.004	.07
Age	.026	3.84E-04	.56
R^2	.29		
Number of cases	11,973[c]		
Church attendance[d]			
Constant	4.385	.113	—
Household size	.025	.008	.03
Age	.007	8.34E-04	.08
No religion	-1.755	.056	-.24
Catholic	.622	.028	.17
Jewish	-.737	.072	-.08
Male	-.397	.024	-.13
Married	.248	.028	.08
Civil rights	.146	.088	.01
Study year	-.023	1.38E-03	-.14
Vietnam cohort	-.162	.038	-.04
Black	.305	.044	.06
R^2	.18		
Number of cases	14,441[c]		
Strength of partisanship[e]			
Constant	2.485	.025	—
Age	.010	4.89E-04	.18
Civil rights	.459	.053	.07
Vietnam cohort	-.121	.025	-.04
1968–80	-.173	.015	-.09
R^2	.05		
Number of cases	16,458		
Civic competence[f]			
Constant	-2.646	.065	—
Family income	.018	1.30E-03	.12
Age-squared	-2.60E-04	2.37E-05	-.49
Age	.035	.002	.68
Male	.291	.014	.17
Years of education	.081	.003	.35
Some college	.434	.042	.24
Age × some college	-.006	9.36E-04	-.14
Woman × college graduate	.084	.031	.02
Self-employed or managerial	.076	.025	.02
Professional or technical	.076	.025	.03
Church attendance	.042	.004	.08
Strength of partisanship	.099	.007	.11
R^2	.32		
Number of cases	12,426[g]		
Government responsiveness[h]			
Constant	.907	.024	—
Black	-.104	.023	-.04
1968–80	-.390	.014	-.24
Years of education	.044	.002	.20

TABLE 1 (*continued*)

Variable	b	s_b	Beta Weight
Party in power	.031	.008	.03
Civic competence	.205	.009	.21
R^2	.17		
Number of cases	12,699[g]		
Family income[i]			
Constant	-16.722	.095	—
Post-1972	14.531	1.267	1.18
Study year × post-1972	-.203	.018	-1.28
Study year	.167	.014	.25
Household size	.429	.029	.12
Age	.311	.015	.86
Age-squared	-.003	1.63E-04	-.82
Retired	-1.163	.173	-.06
Years of education	.506	.017	.29
Self-employed or managerial	2.968	.165	.13
Professional or technical	.983	.169	.05
Skilled	.423	.162	.02
Black	-1.735	.147	-.08
College graduate	1.327	.167	.08
Single woman	-2.813	.113	-.19
R^2	.34		
Number of cases	13,737[e]		
Voting participation[j]			
Constant	-.602	.103	—
Age-squared	-4.54E-04	3.48E-05	-.66
Age	.054	.003	.80
Marital status	.177	.022	.07
Years of education	.049	.004	.15
Some college	.371	.061	.15
Age × some college	-.010	1.35E-03	-.18
Community attachment	.188	.014	.13
Church attendance	.074	.006	.10
Strength of partisanship	.148	.010	.13
Civic competence	.330	.013	.25
Government responsiveness	.112	.012	.08
R^2	.26		
Number of cases	10,579[c,g]		

Note: $p < .01$ for all estimated coefficients except for civil rights in the regression for church attendance, where $p < .10$.

[a] Range = -1.84–5.95; mean = 0; standard deviation = .78.

[b] Values .001 or smaller are presented in scientific notation.

[c] Sample sizes are small because indicators of community attachment and household size were not present in the 1956 NES.

[d] Range = 1–5; mean = 3.18; standard deviation = 1.53.

[e] Range = 1–4; mean = 2.86; standard deviation = .99.

[f] Range = -2.16–1.98; mean = 0; standard deviation = .86.

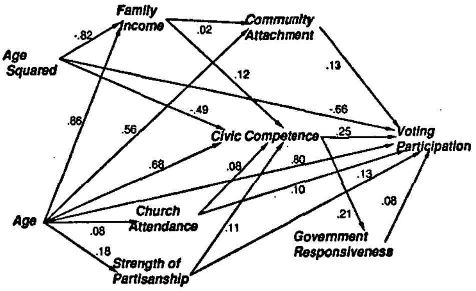
[g] Sample size is small because 1972 civic competence measure includes only 1,119 Form 1 respondents.

[h] Range = 0–2; mean = 1.20; standard deviation = .82.

[i] Range = 0–34.625; mean = 7.88; standard deviation = 5.91.

[j] Range = 0–3; mean = 2.15; standard deviation = 1.15.

Figure 3. Causal Model of Voting Participation Showing All Age-related Paths



Note: Values shown are beta weights. For clarity, the figure deletes exogenous variables other than age and age-squared and the paths from these exogenous variables. (See Table 1 for the values of these path coefficients.) Also, it does not show errors, the paths from errors to endogenous variables, or paths representing unanalyzed correlations between exogenous variables.

the reasons underlying the specification of the age-related effects within the model for each endogenous variable and the results of testing the model.

Civic Competence

Civic competence is the central concept in our theory of life span civic development. Civic competence is defined as knowledge and habits of knowledge acquisition relevant to politics.[7] Making informed political judgments requires political knowledge and habits of attentiveness to politics to assure that knowledge is current. Those who possess greater civic competence are more likely to participate in politics, because they have a better understanding of it and thus are able to make discriminating judgments.

The acquisition of civic competence is an age-related process. For young persons, major sources for acquiring political knowledge include the family, the peer group, and the school. Studies of the process of political socialization of children and adolescents have found that family and school experiences are critical to the development of citizen-related attitudes (Conway 1985; Dawson, Prewitt, and Dawson 1977; Jennings and Niemi 1975, 1981).

Learning about politics, however, does not end with the advent of adulthood. For adults a primary source for acquiring political knowledge is the mass media. Attention to politics through the mass media and, in particular, reading about politics is a hallmark of civic competence. As a method of acquiring political knowledge, reading is superior to television viewing, in part because it is associated with habits of monitoring and thinking about politics that allow for more discriminating judgments (Elder et al. 1985; Wagner 1983). Political knowledge is also acquired in other ways through everyday experience that accumulates with chronological age.

Such experience is particularly important for those who are of lower socioeconomic status and are more likely to leave school early. Numerous studies have shown that political participation is low among the young of all socioeconomic levels, but it is lowest among those in the least-well-off strata (Milbrath and Goel 1977). Wolfinger and Rosenstone (1980), using census data, found that voting turnout among those with the highest levels of education did not increase very much with age. In their study the best-educated started voting at a high rate (about 80%) and increased slightly over the life span (to about 90%). The least-well-educated started voting at a very low rate but made large gains over the adult life span (from about 20% at age 18 to over 50% in old age). The authors conclude that "Life experience is a substitute for school. Many uneducated people, who have the fewest politically relevant skills when they become eligible to vote, become more accomplished in coping with bureaucratic hurdles and thinking about political material. Consistent with this assertion is the previously noted increase in political interest, information, and the use of the mass media among older people without the benefit of a college education and the lack of any such change for those who have gone to college" (p. 60).

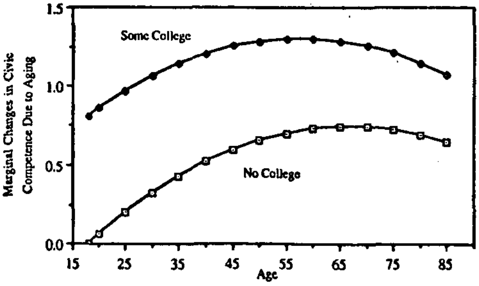
Consistent with this argument, we hypothesize that the accumulation of political experience as a result of growing older leads to increasing levels of civic competence and, in turn, to greater voting participation.

Civic competence, however, may not accrue in steady fashion throughout the adult life span. For young adults, the political learning curve may be quite steep, while for the middle-aged, who are much more knowledgeable already, it may be much flatter. For very old persons there may even be some declines in civic competence due to health problems, hearing loss, and poorer eyesight that make it difficult to pay attention to politics. In order to represent these nonlinear effects of aging we include both age and age-squared as exogenous variables with paths to civic competence.

A complication arises in the possible interaction between age and a college education. We would argue that there is a threshold effect. Those with some college education begin with higher levels of civic competence, even controlling for the effects of number of years of education and other variables, so that age-related gains are less pronounced for them. There may be several reasons for this. Those who enter college generally have stronger academic backgrounds than those who do not and probably know more about politics to begin with. In addition, a college education, particularly in the liberal arts, encourages greater vigilance in monitoring, and thinking about, politics. It also provides an opportunity to learn firsthand about large-scale, bureaucratic institutions and how they function. To represent this threshold effect we include as control variables a dummy variable (0 = no college; 1 = at least some college) to allow for a different intercept and an interaction term that is the product of this variable times age, to allow the form of the curvilinear relationship to vary.[8]

We determined the total changes in civic competence due to aging effects for both those with no college and those with at least some college by summing the direct effects of age with three indirect effects; those of family income, church attendance, and strength of partisanship. Figure 4 shows these changes graphically. For persons with no college there is a substantial increase in civic competence due to aging effects, with the increases peaking at age 67, after which there is a slight decline. For persons with at least some college, however, the increases are less pronounced and peak at a younger age. The gap between the upper and lower curves represents the relative advantage in civic competence at different ages accruing to those with at least some college due to their initial educational advantage. This relative advantage is large for young adults. Although it never disappears, it gradually diminishes with age. Thus, for

Figure 4. Age-related Changes in Civic Competence



Note: We assume a difference of 5.3 years of education between those with at least some college and those with no college.

persons with no college education aging effects cause substantial gains in civic competence that serve to erode their initial disadvantage.

Aside from these effects, we hypothesize that three of the age-related endogenous variables in the model affect civic competence: church attendance, strength of partisanship, and family income. Church attendance leads to greater civic competence largely through the communal and social commitments emphasized in church life. Strength of partisanship affects civic competence in that it is a manifestation of interest in politics and results in both greater attentiveness to, and knowledge of, politics. Family income has positive effects on civic competence because of the wherewithal, in terms of time, and resources, to participate in social life and attend to politics in the mass media. As the coefficients for these paths in Table 1 show, all three of these age-related variables contribute to civic competence.

Community Attachment

Another age-related process is the tendency for persons to become more attached to their local communities as they marry, purchase a house, raise a family, send their children to local schools, socialize with neighbors, and participate in locally based activities and groups. All of these activities lead to a heightened sense of place, increased social investment, and a larger stake in a community resulting in a closer linkage of personal concerns to politics (Verba and Nie 1972). We thus hypothesize that aging is a cause of increased community attachment and, in turn, of greater political participation (cf. Knoke and Thomson 1977; Milbrath and Goel 1977). Since community attachment reflects a commitment to a location, it should vary directly with age in a linear fashion.[9] As Table 1 shows, the effects of age on community attachment are substantial in relation to the effects of typical increases in family income and household size.

Church Attendance

Adults tend to attend church more frequently as they grow older. For the majority participation in church activities is the most common form of social involvement beyond the immediate family. This involvement fosters the acquisition of social skills that contribute to civic competence. A church can be also an important reference group, increasing the perceived salience of some political candidates and issues. Perhaps even more importantly, church attendance involves a sense of personal affiliation with an institution in which communal values and social obligations are regularly emphasized. These values may be a motive force for other social and community commitments, leading to increased civic competence and voting participation (Bellah et al. 1985; Knoke and Thomson 1977). Church attendance may also reflect a tendency for involvement in local community groups, a variable that has a positive effect on voting participation but for which we lack a measure (Elder et al. 1985; Verba and Nie 1972).

We hypothesize that church attendance increases with age due mainly to life-cycle effects associated with family life and social responsibilities, habit formation, and the need for social interaction. For many persons a church is the institutional arena within which is found the meaning and significance of such events as marriage, childbirth, childrearing, the marriage of one's children, and death (Bellah et al. 1985). As Figure 3 shows, however, the effects of age on church attendance are small relative to the effects of other exogenous variables.[10]

Strength of Partisanship

Yet another age-related process arises from the strengthening of party attachments, or the loyalties people acquire toward particular political parties. They reflect psychological identification and can "persist without legal recognition or evidence of formal membership and even without a consistent record of party support" (Campbell et al. 1964). Many individuals in stable democracies acquire party attachments when they are young, often from their parents. Some who do not acquire party attachments when young do so later, typically as young adults. Although the party attachments of adults are subject to a variety of influences, in political systems with a stable party system they tend to become stronger as a person grows older as a "direct function of [their] continuing experience with the party system" (Converse 1972, 73). The passage of time offers people repeated opportunities to support verbally, and vote for, a party and its candidates, resulting in learning by means of psychological reinforcement.[11] Strong party attachments lead to greater psychological involvement in politics (Campbell et al. 1964; Converse 1976). Greater psychological involvement, in turn, leads to higher levels of political participation. Consistent with earlier findings, as shown in Figure 3, there is a fairly strong relationship between age and strength of party attachments in our model.

Government Responsiveness

Government responsiveness (oftentimes called "external political efficacy") is the feeling that political participation will make a difference because public officials will actually pay attention to it. The stronger this feeling, the greater the motive to vote. In the model this effect is represented by the direct arrow from government responsiveness to voting participation. In our model there is a path from a single endogenous variable, civic competence, to government responsiveness. Those who are more knowledgeable about politics, we would argue, have a better understanding of government and are more likely to appreciate participation's impact on it. Therefore, age acts on feelings of government responsiveness through its impact on civic competence.

Family Income

The effects of age on family income are nonlinear. Most people's family income (in constant dollars) tends to increase throughout their adult life span until the loss of earnings through disability or retirement. Reasons for this include increases in earnings due to greater seniority, promotions, and increased productivity in the economy generally. These increases tend to be greater at younger ages. The decrease in family income from the loss of earnings through disability or retirement (or partial retirement) depends on the extent to which such earnings are replaced through social security benefits, private pensions and other sources and on tax considerations (Schulz 1980).

Family income affects voting participation through the intervening variables of community attachment and civic competence. Family income has a positive effect on community attachment because it provides the means to buy a home and establish a stable pattern of residency. It also has a positive effect on civic competence for the reasons previously mentioned.

Voting Participation

The causes of voting participation are numerous and include, in addition to the effects of a number of exogenous variables that we control for in the model, the effects of aging processes represented in the model by paths originating from age and age-squared and going either directly or indirectly to voting participation. The direct paths from the surrogate variables age and age-squared to voting participation represent the effects of variables not included in the model due to data set limitations—such variables as citizen duty, health, and disability.

The development of a sense of citizen duty, we would argue, is in part the product of the process of habit formation. Many habits, including voting habits, allow persons to respond in routinized and socially approved ways to recurring environmental events (Dahl 1956). The election of a president every four years is such an event. As people show up at the polls to vote and receive social approval for fulfilling an obligation of citizenship, they develop voting habits and an increased sense of citizen duty. Thus, in the model the variable of age is a surrogate for the development of voting habits and is a direct cause of political participation quite apart from any gains in civic competence.

Age-related gains in voting participation should be smaller for those with some college education than for those with none. This is because their sense of citizen duty and rates of voting participation are already higher to begin with due to the socialization effects of institutions of higher education. In the model we account for this relationship by including the same two control variables (i.e., some college; age times some college) as we did with civic competence.

Age and age-squared are also surrogates for varying levels of health and disability. While most older persons are relatively healthy and live independently in the community, overall they have poorer health than do middle-aged and younger persons. Health is a major concern of persons 65 and older (National Council on the Aging 1975, 1981). Siegel and Davidson (1984) have summarized data that explains how health affects the mobility of older persons. They report that while those over 65 years of age have about one-half the incidence of acute care episodes each year than do those under that age, the likelihood that an older person will suffer from a chronic health problem is much higher than for those who are younger. This likelihood increases with increasing age. About 11% of the general population experiences limitations in major activity, but this varies from 4% (for those under 45 years of age) to 19% (for those between ages 45 and 64) to 39% (for those over 65 years of age). About 86% of those over 65 years of age suffer from at least one chronic health problem, with multiple conditions commonly being found among the oldest elderly (those 75 years of age and older). Problems of disability and poor health among older persons, not surprisingly, depress their levels of participation in social life, including political participation (Peterson 1987). Thus, declines in voting participation among the elderly are probably attributable to declining health and increasing disability that make it difficult for some of them to go to the polls.

The indirect effects of aging are represented by the paths originating from age and age-squared and going through one or more of the six endogenous variables with effects on voting participation. The most substantial indirect effect of aging occurs because of the tendency for civic competence to increase with age. Another substantial indirect effect results from the tendency for people to become more strongly attached to their local communities as they grow older.

The tendency for family income to increase with age has a moderate impact on voting participation. There are three indirect paths from age and age-squared through family income to voting participation: one through community attachment, the second through civic competence, and the third through civic competence and government responsiveness.

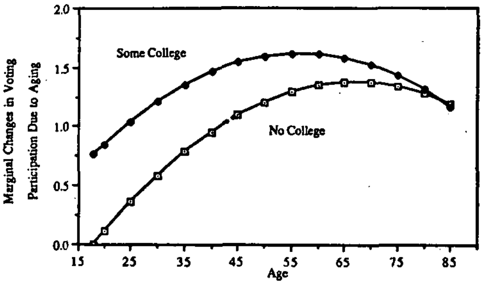
Another notable indirect effect of aging arises from the tendency for party attachments to get stronger as people grow older. There are three indirect paths from age through strength of partisanship to voting participation: one through strength of partisanship alone, the second through civic competence, and the third through civic competence and government responsiveness.

The least substantial indirect effect results from people's tendency to attend church more frequently as they grow older. In the model there are three indirect paths from age through church attendance alone, the second through civic competence, and the third through civic competence and government responsiveness.

The indirect effects of age on voting participation vary with age. Altogether, looking at the age at which voting participation reaches a maximum, about one-half[12] of the effects of age operate indirectly through the variables we have identified in our theory of life span civic development. Indeed, there is a probable bias in the estimation of these indirect effects due to measurement errors in civic competence and the other age-related variables. With better measures we would expect to see even stronger indirect effects of age on voting participation.

Figure 5 shows the changes in voting participation due to the total effects of aging graphically both for those with no college and those with some college. These were determined by summing both the direct and indirect effects. For those with no college, increases in voting participation due to aging effects are substantial and peak at age 67; for those with at least some college, however, increases are less, with a peak at a much-younger 57 years of age. For those with no college, aging effects continue to impinge positively on voting participation into late age. For persons who have attended college, however, the positive effects of aging are counterbalanced in late middle age by its negative effects, those of declining family income, poorer health, and increasing disability.

Figure 5. Age-related Changes in Voting Participation



Note: We assume a difference in 5.3 years of education between those with at least some college and those with no college.

Conclusions

That political participation in general and voting participation in particular tend to be greater among persons with higher socioeconomic status is a well-established proposition. Age-based variation in participation is similarly well known. While the research that has demonstrated these relationships has been largely cross-sectional, it is generally accepted that these patterns reflect age-related individual development.

There is a potential fallacy, of course, in making inferences on the basis of cross-sectional data. Ideally, one would like to have panel data over an extended period of time to verify these relationships and to estimate the effects of specific variables. In the absence of such studies, the only way to guard against fallacious inferences from cross-sectional studies is to model the processes involved by taking into account confounding influences from period effects, cohort effects, and compositional changes in the sampled population. To this end we have created a cross-sectional time series data set and have modeled the dynamics of life span civic development as it impinges on voting participation.

Consonant with the findings of cross-sectional studies and hardly surprising, both age and education were found to play a critical role in explaining voting participation. However, our analysis disentangles the dynamics of these and other relationships by exploring the mediating processes involved. Since for most people the level of formal education is fixed by the time they become adults, we sought to sort out why it is that most modes of participation increase with age and, in the case of voting participation, decline only slightly in later life. We found that age-related changes in five variables were useful in illuminating the dynamics involved: increasing community attachment, more-regular church attendance, marginal increases in strength of party attachments, changes in government responsiveness, and, most importantly, increasing civic competence. Indeed, with these variables we were able to account for almost one-half of the variation in voting participation attributable to aging effects. We are convinced that with better measures of the concepts employed here we could explain an even greater portion of the age-based variation. There are, of course, other dynamics involved in life span civic development, such as changes in health, disability, and citizen duty that could not be explicitly included in our present model.

We believe that the findings with respect to civic competence are particularly significant. As we conceive it, civic competence comprehends an interest in and attentiveness to politics, contextual knowledge about politics, and habits of monitoring politics that provide the type and detail of information needed to sustain informed judgments. We found that the various elements of this conception do indeed covary to form a unidimensional property. As might be expected, education (directly and indirectly) contributes substantially to civic competence, as does income. But more importantly, civic competence is a function of aging effects quite independent of education and income. Thus, life experiences do provide a functional substitute for formal education in the development of civic competence and have the effect of dampening the biases in patterns of political participation attributable to socioeconomic status. Life experiences, it would seem, not only enhance the level of political participation but have their most profound effects on those otherwise least advantaged.

Appendix A: Measurement of Variables

Exogenous Variables

1. *Age (in years)*
2. *Age × some college.* An interaction term equal to age *times* some-college variable
3. *Age-squared.* A polynomial introduced to allow for the estimation of curvilinear relationships
4. *Black.* A dummy variable, 1 for a black respondent, 0 for all other persons
5. *Catholic religion.* A dummy variable, 1 if respondent is a Catholic, 0 otherwise
6. *Civil rights.* A dummy variable, 1 for black non-Republican respondents interviewed in the 1960, 1964, or 1968 NES studies, 0 otherwise
7. *College graduate.* A dummy variable, 1 if the respondent has four or more years of college (i.e., is a college graduate), 0 otherwise
8. *Household size.* The number of persons living in the household (excluding children over the age of 18)
9. *Jewish religion.* A dummy variable, 1 for respondents who are Jewish, 0 otherwise
10. *Male.* A dummy variable, 1 if respondent is a man, 0 if a woman
11. *Marital status.* A dummy variable, 1 if respondent is married, 0 otherwise
12. *1968–80.* A dummy variable, 1 for respondents interviewed during one of these years, 0 otherwise
13. *No religion.* A dummy variable, 1 for respondents who claim no religious

affiliation or who claim to be atheists or agnostics, 0 otherwise

14. *Party in power.* A variable, 1 if the respondent holds a party attachment and the incumbent president is a member of the respondent's party, 0 if the respondent is an independent, and −1 if the respondent holds a party attachment and the incumbent president is a member of the other party

15. *Post-1972.* A dummy variable, 1 for the 1972–84 NES, 0 for other years (the trend in average family income [constant] dollars from 1952 through 1984 can be decomposed into two separate trends: an upward trend from 1952 through 1968 and a much flatter trend from 1972 through 1984; the post-1972 variable allows for a different intercept for the 1972–84 trend

16. *Professional or technical occupation.* A dummy variable, 1 if the respondent holds a professional or technical occupation, 0 otherwise

17. *Retired.* A dummy variable, 1 if the respondent is retired, 0 otherwise.

18. *Self-employed or managerial occupation.* A dummy variable, 1 if the respondent is self-employed or held a managerial occupation, 0 otherwise

19. *Single woman.* A dummy variable, 1 if the respondent is a woman who is single, divorced, separated, or widowed, 0 otherwise

20. *Skilled occupation.* A dummy variable, 1 if the respondent has a skilled occupation, 0 otherwise

21. *Some college.* A dummy variable, 1 for respondents with at least one year of college, 0 for those with no college

22. *Study year.* A variable equal to 52, 56, 60, and so on for the NES study year to account for a linear trend over the period 1952–84

23. *Study year × post-1972.* An interaction term equal to study year variable *times* post-1972 variable (this variable allows for a different slope for the 1977-84 trend)

24. *Vietnam cohort.* Constructed from age at the time of the interview, a dummy variable, 1 for respondents born between 1945 and 1957 inclusive, 0 otherwise

25. *Woman × college graduate.* An interaction term equal to a gender dummy variable (1, woman; 0, man) *times* college graduate variable (that is, woman who has four or more years of college is scored 1, all others 0)

26. *Years of Education*

Endogenous Variables

1. *Church attendance.* This measure is based on the self-reports of respondents. Response categories varied slightly over the years (through 1968: *regularly, often, seldom, never;* beginning in 1972; *every week, almost every week, once or twice a month, a few times a year, never).* Responses through 1968 were assigned values of 5, 4, 2, and 1, respectively; responses from 1972 were assigned values of 5, 4, 3, 2, and 1, respectively. Respondents who claimed no religious affiliation (and who were not asked a follow-up question on church attendance) were coded as *never.*

2. *Civic competence.* We saved factor scores after conducting confirmatory factor analysis for each of the nine studies (see Appendix B) to estimate the construct of civic competence, a single factor, hypothesized to be the cause of as many as seven observed indicators. These include a single measure of the respondent's attentiveness to politics (a question on whether the respondent follows politics regularly or not), two measures concerning reading about politics (read about campaign in magazines; read about campaign in newspapers), and four measures of contextual knowledge and cognitive skills (which party controls the House of Representatives before the election, which controls the House after the election, the interviewer's assessment of the respondent's level of political information, and a ques-

tion about which party is the more conservative party).

3. *Community attachment.* We conducted confirmatory factor analysis for eight of the nine studies (see Appendix B), setting out the hypothesis that the unobserved construct of community attachment, a single factor, is the cause of three observed indicators: homeownership (a dummy variable, 1 if the respondent owned a home, 0 otherwise), length of residency in a home (measured in years), and length of residency in a community or state (measured in years).

4. *Family income ($000).* We assigned respondents to the midpoint of their class interval, except respondents in the highest income group, who were assigned a family income of 120% of the lower boundary. We used the consumer price index (1967 = 100) to adjust for inflation.

5. *Government responsiveness.* The NES have asked four different questions continuously to assess political efficacy (Madsen 1987). Political efficacy is "the feeling that individual political action does have, or can have, an impact upon the political process . . . the feeling that political and social change is possible, and that the individual citizen can play a part in bringing about this change" (Campbell, Gurin, and Miller 1954, 187). Of these four questions (in which respondents are asked to agree or disagree), we will use two that conceptually reflect external political efficacy, or what we call government responsiveness: "People like me don't have any say about what the government does" and "I don't think public officials care much what people like me think." We constructed an index varying from 0 to 2 of the number of efficacious responses (*disagree*) to these two questions.

6. *Strength of partisanship.* The NES summary party identification measure was used to construct an index ranging from 1 (apoliticals, pure independents) to 4 (strong party attachments).

7. *Voting participation.* This is an index varying from 0 to 3 reflecting the number of the three electoral activities (registration, voting in the previous presidential election, voting in the current presidential election) in which the respondent reported engaging. Persons not registered were coded as 0. Persons only registered were coded as 1, except that persons age 25 (22 from 1972 on) and older were coded as 1 if they were not registered but had voted in the previous presidential election. Persons age 25 (22 from 1972) and older who were registered and voted in either the previous or the current presidential election but not both were coded as 2. Persons age 25 (22 from 1972) and older who engaged in all three activities, as well as those under 25 years old (22 from 1972) who were registered and voted in the current election were coded as 3. Note that in the 1984 NES there was no question regarding voting in the previous presidential election. We imputed values for this variable randomly, using the binomial distribution. Using marginal distributions from the 1980 NES, we generated probabilities of voting in the previous presidential election based on whether the respondent was registered and whether the respondent voted in the current presidential election.

Appendix B: Rationale for Exogenous Control Variables

1. *Civic competence.* Control variables with direct effects on civic competence include gender, years of education, being in a manager or self-employed occupation, and having a professional or technical occupation. For obvious reasons, the effects of education on civic competence should also be positive. We also expect that men, controlling for the effects of other variables, will have higher civic competence than women, mainly because of the persistence of traditional sex roles that lead men to take a greater interest in, and pay more attention to, politics. Finally, manager or self-employed and professional or

technical occupations should be associated with higher civic competence. These occupations generally require the use of verbal skills and offer greater autonomy and responsibility and greater opportunities to discuss politics with coworkers, clients, customers, and others in the workplace. In the model there are no period effects on civic competence. We would argue that civic competence is a relatively durable attribute and much more stable over time than such psychological orientations as political trust, which involves evaluations of current political actors and events (Citrin 1974; Miller 1974). Accordingly, the effect of civic competence on political participation is likely to be more stable and less contextually dependent than that of psychological orientations.

2. *Church attendance.* Ten control variables—*household size, no religion, Catholic religion, Jewish religion, male, marital status, civil rights, study year, Vietnam cohort,* and *black*—affect church attendance. Catholics, in part because of the emphasis placed on this obligation, attend church more frequently than do Protestants and more frequently than Jews attend synagogue. Those with no religious affiliation seldom go to church. Other characteristics also affect church attendance. Women attend church more often than men, largely because of their historically heavier involvement in family life and the religious instruction of children. In addition, church attendance is higher among persons from large households, perhaps because of the emphasis many parents give to assuring that their children receive religious instruction.

We hypothesize that increasing secularism over the entire 32-year period (*study year*) led to a general decline in church attendance. It is likely that the Vietnam cohort, who grew up during this period, was particularly affected by this trend. Finally, we expect elevated church attendance among blacks generally, because of the special significance of the church in the community life of many blacks, and among black non-Republicans during the 1960s especially, because of the leadership role assumed by black clergy in the civil rights movement.

3. *Community attachment.* In the model, there is a direct path from a single control variable—household size—to community attachment. Household size is positively correlated with number of children under the age of 18, family income, involvement in local schools, and, presumably, community attachment.

4. *Family income.* In the model, there are 12 control variables affecting family income. There was substantial growth in average family income (in constant dollars) from 1952 through 1972. Thereafter, growth in average family income flattened. Three variables including *post-1972, study year* × *post-1972,* and *study year* are used to detrend this series. A number of control variables have positive effects on family income. Years of education has a positive effect on family income because of its association with increased earnings, typically the most important component of family income for nonretired persons. There is a threshold effect for college graduates since a degree enables them to enter more prestigious occupations and receive faster promotions. Earnings in self-employed or managerial, professional, and skilled occupations are generally higher than earnings in other occupations. Other control variables have negative effects on family income. Retirement is generally associated with a decline in family income because of the failure of social security benefits, pension benefits, and other sources of income to replace losses in earnings. Blacks have lower family incomes than others, controlling for the effects of other variables, due mainly to historical patterns of racial discrimination in education, employment, and housing. Women who live alone (i.e., single, divorced, separated, widowed) are also relatively disadvantaged. A disproportionate number of the employed work in low-paying occupations (e.g., typist,

waitress, sales clerk) in low-paying industries (e.g., food service, retail trade). Some with young dependents must rely on inadequate AFDC benefits. Many widows depend almost entirely on inadequate social security benefits.

5. *Government responsiveness.* In the model there are four control variables with paths to government responsiveness. We hypothesize that blacks, as an historically disadvantaged group, have more negative evaluations of governmental responsiveness than do nonblacks. *Years of education* is positively associated with government responsiveness due, we believe, to a better understanding by persons with more education of the scope and purpose of government activity. We also hypothesize a period effect from 1968 to 1980 that lowered evaluations of government responsiveness (Abramson and Aldrich 1982; cf. Madsen 1987). We would argue that the period effect was largely spent by the time of the election of Ronald Reagan in 1980. Many thought during Reagan's early years in office that his election was a fresh start that marked the end of a period of turmoil and lack of a sense of political direction. Another control variable with an effect on government responsiveness is party in power. We hypothesize that Democrats evaluate government more positively when a Democrat is president, more negatively when a Republican is president. Republicans should do the opposite.

6. *Strength of partisanship.* Three control variables—the Vietnam cohort, the period 1968-80, and a period effect for black non-Republicans during the 1960s (*civil rights*)—affect strength of partisanship. We hypothesize lower strength of partisanship among members of the Vietnam cohort due mainly to depressed levels of partisanship transmission and the tumultuous period of political history when this cohort came of age. This history—which included the riots in the cities (1967), the Vietnam war (1965-75), the political assassinations of Robert Kennedy and Martin Luther King (1968), the Chicago convention (1968), and Watergate (1972-75)—discouraged partisanship among the young of this generation. We also hypothesize a lengthy period effect from 1968 to 1980 because of political events that served to weaken party attachments among all persons, regardless of their age. These include the events mentioned, as well as urban riots, disorders on college campuses, the OPEC oil shocks and stagflation, and the taking of hostages in Iran. In addition, the party attachments of black non-Republican became stronger during the 1960s because the national Democratic party took the lead in formulating and enacting landmark civil rights legislation (Converse 1976).

7. *Voting participation.* There are a number of control variables with effects on voting participation: *marital status, some college,* and *age* × *some college.* Voting participation should be higher among married persons, and the disproportionate number of widows and widowers among older persons should lower their rate of voting participation relative to other age groups. Voting participation should also be higher among persons with more education, due to their greater sense of citizen duty.

Appendix C: Civic Competence and Community Attachment Scales

For the following tables we obtained factor loadings using confirmatory factor analysis. We selected the indicators based on the results of an exploratory factor analysis using 1976 NES data in which all of the named indicators loaded positively on a single factor, with other indicators such as television viewing, loading on other factors. We set initial communalities equal to the multiple correlation coefficient and restricted the solution to a single factor so that there was no rotation. In 1952 and 1956 there are only two indicators of civic competence, so that, using factor analysis, the loadings are constrained to be the same. The loadings are not that different, however, from those in other years. We conducted factor analysis for years 1960–84 using only two indicators, saved factor scores, and correlated these factor scores with those obtained using all of the indicators. The two sets of factor scores, for all of these years, were highly correlated (> .90).

Table C-1. Factor Loadings for Civic Competence Scale

	Year of U.S. National Election Study								
Indicators	1952	1956	1960	1964	1968	1972	1976	1980	1984
Follow politics[a]	—[b]	—	.58	.67	.75	.67	.71	.64	.54
Read magazines[c]	.65	.59	.54	.51	.51	.46	.60	.53	.52
Read newspapers[d]	.65	.59	.64	.59	.59	.57	.68	.65	.63
Party controlling House before election[e]	—	—	.65	.60	.56	.64	.67	.48	.66
Party controlling House after election[f]	—	—	.43	.58	.43	.54	.62	.32	.61
Interviewer's assessment[g]	—	—	—	—	.79	.71	.74	.77	.70
Party more conservative[h]	—	—	.49	.50	.50	.41	.52	.46	—
Eigenvalue	.84	.69	1.90	2.00	2.55	2.36	2.96	2.26	2.25
% variance	41.9	34.4	31.6	33.3	36.4	33.7	42.3	32.2	37.5
Number of cases	1,703	1,749	1,084	1,422	1,308	1,086	1,816	1,376	1,918

[a] Respondent's political interest.

[b] No NES question.

[c] Frequency with which respondent read about campaign in magazines.

[d] Frequency with which respondent read about campaign in newspapers.

[e] Whether or not respondent knew party that controlled the House of Representatives before the election.

[f] Whether or not respondent knew party that controlled the House of Representatives after the election.

[g] Interviewer's assessment of the political knowledge of the respondent.

[h] Whether or not respondent knew which party is more conservative.

Table C-2. Community Attachment Scale

	Year of U.S. National Election Study								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Length of residence (community)[a]	.48	—[b]	.68	.47	.41	.40	.65	.66	.64
Length of residence (home)[c]	—	—	.68	.75	—	—	.91	.94	.94
Home ownership[d]	.48	—	—	.36	.41	.40	.31	.30	.34
Eigenvalue	.46	—	.92	.91	.34	.32	1.34	1.42	1.41
% variance	22.9	—	46.1	30.5	16.8	16.2	44.6	47.3	47.2
Number of cases	1,783	—	1,137	1,512	1,488	2,590	2,209	1,603	2,229

Note: For 1952, 1960, 1968, and 1972 there are only two indicators of community attachment. In comparison to the civic competence scale there are fewer indicators and greater variability in the magnitude of factor loadings across time. Thus, measurement error for this scale is likely to be greater than that for civic competence. In future research, additional indicators will be needed to obtain a better measure of community attachment. Among these would be indicators of local community group involvement, which are unavailable in most of the NES.

[a] Length of residence in a community (or state), in years.

[b] No NES question.

[c] Length of residence in a home, in years.

[d] A dummy variable, 1 if respondent owns a home, 0 otherwise.

Notes

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1. The NES series provides data on a regular basis through surveys nationally administered by the Institute for Social Research (ISR) of the University of Michigan during national election periods. These surveys have been carried out by ISR since 1952.

2. Cutler (1977) and Edinger (1983) discuss the political implications of aging populations in Western democracies.

3. The NES series are probability samples and are representative of citizens of voting age. The samples were drawn using a multistage cluster-sampling technique, and unweighted sample sizes vary from a low of 1,181 in 1960 to a high of 2,705 in 1972.

4. We include both age and age-squared in several of the equations, a polynomial of the second degree, to specify the nonlinear (single-bend) effects of age.

5. We do not focus here on the many contextual variables (e.g., registration laws, types of elections, governmental structures, organizations, and groups) at the aggregate level that are potential causes of individual-level variations in voting behavior (see Verba, Nie, and Kim 1978).

6. Cross-sectional studies are limited in that they can only analyze the impact of age differences at one period in time. Generalizing about developmental processes on such a basis is especially risky. Such studies are subject to the confounding of aging, cohort, and period effects, which cannot be disentangled (Glenn 1977). Most researchers employ cohort analysis using multiple cross-sectional studies (e.g., Cutler and Bengston 1974). The strongest research design in dealing with this problem is the longitudinal study, which follows the same individuals over time. As these are expensive and difficult to implement, they are rare. An example of a particularly strong research design is the Jennings-Niemi three-wave study of 1964 high school seniors and their parents (Jennings 1979; Jennings and Beck 1979; Jennings and Markus 1984, 1988; Jennings and Niemi 1981).

7. This definition should be distinguished from Almond and Verba's (1963) early definition of *citizen competence* that more nearly approximates contemporary definitions of *external political efficacy.*

8. We compared the fit of this model with that obtained by substituting a simple interaction term between *age* and *years of education.* The fit of the latter model was poorer.

9. In further analysis, to allow age to have nonlinear effects on community attachment, we introduced age and age-squared into the regression equation. We found that the marginal effects of age on community attachment increase steadily in nearly linear fashion over the entire adult life span.

10. The marginal effect of age on church attendance increases slowly in nearly linear fashion over most of the adult life span. Although the effect levels

off in an advanced age, there is only trivial nonlinearity, meaning that older persons are more likely to be able to overcome health and physical limitations to get to church than to get to the polls during elections.

11. Converse (1972) argues that the learning may be inhibited in advanced age (the "forgetting effect"), suggesting that age has a nonlinear effect on party attachments. To test this hypothesis we estimated the coefficients of a regression that included both age and age-squared to allow for nonlinearity, but obtained only a trivial improvement in fit. The effect of age was linear throughout a wide range of ages.

12. Calculated as the ratio of the indirect effects to the total effects (indirect plus direct) of aging, from age 18. For persons with no college voting participation peaks at about age 67, and the ratio at that age is .41. For persons with some college it peaks at about age 62, and the ratio at that age is .57. For persons with no college, therefore, a relatively larger proportion of aging effects is attributable to direct effects.

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DOES TRADE STILL FOLLOW THE FLAG?

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A public choice approach is used to build a model of bilateral trade flows employing international conflict and cooperation, as well as prices and income, to predict the level of imports. The model is consistent with neoclassical trade theory in economics, while employing data and measurement insights from the field of international relations to construct an indicator of bilateral diplomatic relations. The model is empirically tested using pooled, cross-section time-series analysis for six nations representing different worlds of development. Considerable empirical support is found for the model in general. The effects of diplomacy on commerce are significant and conform to model predictions in every instance. Estimation results also indicate possible cross-national differences in the political management of trade relations.

The economic ties between any two nations are frequently affected by a warming or cooling of bilateral diplomatic relations, but there is little to be found in modern economics or political science that helps us understand the connections between international politics and commerce. The notion that international economics could be studied in isolation from its political underpinnings or consequences would have been quite alien to mercantilist thinkers in the eighteenth century or to the critics or advocates of imperialism in the nineteenth. That economics and politics were inextricably intertwined was taken as a matter of course. Indeed, "Trade follows the flag" was central to these people's understanding of world politics and international economics. Intellectual trends over the past century radically changed this worldview. The ascendancy of liberalism in economic thought brought with it a compartmentalization of disciplines that may now be eroding, but we are left with theories of international politics and economics that are very weakly bridged.

The objective of this study is to map one of the many connections between international politics and commercial ties. Specifically, I seek to identify and measure the effect of changes in state-to-state diplomatic relations on trade flows between those nations. Theoretical underpinnings of my effort are in the tradition of "public choice" political economy (Frey 1984) because I treat importers as rational utility maximizers and their behavior as determined by preferences pursued within a context of specified constraints.

The model I put forward is wholly consistent with contemporary neoclassical trade theory, but I extend the economists' notion of welfare and contend that importers take account not only of the price and quantity of goods and services but of the place of origin of these products and the political relationship between the importing and the exporting nation. They do so essentially for traditional security reasons; that is, importers are motivated to manage their trade relations in ways that minimize risks of disruption in supply or the possibility of hurting friends or

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aiding foes. All this means that in the aggregate I expect the level of trade between two countries to decline as their relations become more hostile and to increase as their relations become more friendly. By adopting this approach and presenting empirical support for the model, I hope to help bring new substantive research questions from the field of international political economy into a dialogue among the many political scientists and economists who employ the public choice paradigm.

The study reported here builds significantly on work I have reported elsewhere (Pollins 1989; Pollins and Brecke 1987; Pollins and Kirkpatrick 1987). These earlier pieces describe variations of the theoretical model presented here, as well as several empirical tests that strongly shaped the particular specification I chose for estimation. This prior work (which will be described briefly) presented empirical tests that indicated a definite relationship between diplomatic conflict, cooperation, and commerce in a 25-nation trade system (see especially Pollins 1989). I will also report results of several empirical tests, in which the model is employed on independent occasions to explain the level of imports by 6 different nations from each of 24 major trade partners over the years 1955–78.

The six importers chosen as the focus of this study—the United States, the Federal Republic of Germany, the German Democratic Republic, the Soviet Union, Egypt, and India—were selected for reasons of research design. The contemporary global economy is sometimes characterized as encompassing three worlds of development: the worlds of advanced industrial nations, socialist central planning, and developing nations. By selecting two representatives of each world I can test whether the model applies only to certain types or to all. General applicability is by no means a foregone conclusion given the market-oriented, neoclassical foundation of this study. No more than one-third of my selected sample could be considered as avid practitioners of free market ideals.

This design permits additional interesting comparisons. The relationship of foreign trade to the domestic economy differs across these worlds of development, as does the role played by government in managing foreign trade relations. Two representatives of each type were chosen in order to provide a modest means of assessing any possible differences between the three types and to permit controlled division between nations with relatively small foreign sectors (the United States, Soviet Union, and India) and those with sizable foreign sectors (the Federal Republic of Germany, the German Democratic Republic, and Egypt). Though not a central concern of this particular study, this simple comparison may offer insight into any role that trade dependence might play in conditioning the key relationship of interest.

The time series employed in this study begins in 1955. This year marks the founding of the nonaligned movement, in which India and Egypt have played key parts. It was also the first full year in which the German Democratic Republic could be considered a sovereign nation, and many mark it as the year in which postwar reconstruction could be considered complete. The time series ends in 1978 purely for reasons of data availability. Many nations would fit the structure of this design and could have been chosen for this study. Reasons for selecting these particular six will be discussed later.

The theoretical model will be tested by regression analysis for each of the six selected nations. I employ a pooled, cross-section time-series approach, as this permits a larger number of observations to be employed in parameter estimation than cross-section or time-series analysis alone. This advantage is important to my study, given the simple nature of the model and the relatively short time series available.

However, this approach is also subject to estimation difficulties pertaining to nonrandom variation in the residuals. These methodological problems will be discussed briefly in relation to the estimation results.

International Trade and Politics: Key Works

Three bodies of literature most directly inform my effort to specify theoretically and measure empirically the effect of international diplomacy upon trade. Polachek (1980) and Arad and Hirsch (1983) derive the argument that trade inhibits conflict from classical and neoclassical proofs that international trade benefits both parties to the exchange. By adding an assumption that interstate conflict inhibits trade, they demonstrate that utility-maximizing actors will be averse to conflict with their trade partners because such conflict will be damaging to welfare. Considerable empirical support for hypotheses derived from this argument are presented in Polachek 1980 and Gasiorowski and Polachek 1982. While the main focus of their work differs from the present study, the model and empirical results I will put forward are directly relevant to the key assumption made by this school, namely, that bilateral conflict inhibits trade.

A second research tradition is grounded in "transaction flow analysis," developed by Richard Savage and Karl Deutsch (1960). The essential idea of this work has led some to hypothesize that nations with exceptionally cooperative political relations would engage in more trade, while conflictive nation pairs would be expected to trade less (Kunimoto 1977; Nagy 1983, 7). This is the very thrust of the model I present here, but no one working in the "transaction flow" school has attempted to measure the effect of interstate conflict and cooperation on trade explicitly. Moreover, Savage and Deutsch (1960, 552) themselves point out that their approach can only be used as a descriptive device because of its weak grounding in economic theory (see also Mayes 1978, 10; Tollison and Willet 1973). I seek a fuller grounding in contemporary trade theory than the Savage-Deutsch approach provides, although the relevance of their efforts to my theoretical and empirical interests is clear. I see my study to be entirely consistent with their original idea.

A third important body of work attempts to measure the effect of institutionalized political-economic cooperation on trade flows. In this approach, such free trade areas as the European Community or the Central American Common Market are incorporated into import demand equations by use of dummy variables (Aitken 1973; Brada and Mendez 1983; Pelzman 1977). Of course, the use of dummy variables means there is no attempt to distinguish highly cooperative nation pairs from weakly cooperative nation pairs. Moreover, cooperation outside the context of the free trade association is ignored, as is all international conflict. I want to take this extra step and measure the effects on trade of all forms of bilateral cooperation and conflict. While this dummy variable approach is not adequate for my purposes, the solid grounding of these models in empirical trade theory is an important starting point. Indeed, my earlier empirical work in this area did build directly on these models (Pollins 1989), and the results of those estimation exercises shape the model described.

In sum, the present effort is assisted by a variety of existing approaches found in political science and economics. Polachek's work demonstrates an application of "public choice" political economy to the study of conflict and trade. This is the general approach I have adopted. Savage and Deutsch point to ways in which changing relations of amity and enmity can affect such international transactions as trade. This is the relationship I will explore. And Aitken opened a door that allows the incorporation of expressly political determinants into models of international trade. This is the threshold I plan to cross.

International Politics and Imports: A Model

I seek to identify and measure the effect of nation-to-nation political relations upon trade flows. I begin by asking how importers choose among various products and different sellers in the international marketplace. In economics this very question is addressed by demand theory. Thus, by looking first to demand theory as it applies to international trade I can begin the model-building process. Once the foundation is set, international political factors may be incorporated and understood in terms of the basic formulation, and the resulting model may then be tested empirically. I will present the construction of the model, discuss its operationalization and the nature of the empirical test, and present the results of those tests.

The Basic Model

Much of modern economic theory is concerned with the question of choice under constrained conditions, and models of social choice are greatly simplified when one assumes that a social actor behaves in a way that maximizes a stated objective. In demand theory the consumer is the actor of interest, and consumers are assumed to make purchases in a way that maximizes their satisfaction within some finite level of expenditure. Many factors may determine satisfaction, but for the sake of simplification these are grouped into a single concept of "utility." Similarly, many considerations may affect the total level of expenditure, but in demand theory this is usually treated as given and referred to as the "budget constraint."

Let us look more closely at the case where an importer must decide how to allocate its purchases (expenditures) among several exporters in the world market. The behavior to be explained (i.e., the dependent variable) is the quantity of imports that a particular importer (i) purchases from a particular exporter (j). This quantity can be denoted M_{ij}. Expenditure is represented by a variable indicating the importer's income, or level of economic activity. Operationally, this may involve an activity indicator that is particularly relevant to the market in question (as manufacturing activity, for example, is important to petroleum demand). Alternatively, a more general indicator (such as gross domestic product) may be employed if imports of all goods are to be explained. By convention, we can denote the income term for a particular importer Y_i. Finally, following the neoclassical concept of welfare, consumer utility is assumed to be maximized when the greatest quantity of goods is purchased for a given level of expenditure. This means that importer behavior will be importantly determined by the prices that exporters offer in the market. To explain the level of purchases from a particular exporter (j), it will be necessary to know the price which that seller offers to the importer (P_{ij}) as well as the prices offered to the importer by all other exporters in the market (P_{i*}).

As Leamer and Stern (1970, 17) point out, researchers usually employ a linear or a log-linear functional form when they define the relationship among these variables. The response, or "elasticity," of the dependent variable to a change in any single independent variable is more directly interpretable when the log-linear form is used, and I adopt that form here.[1] Thus, the basic model of import demand found in contemporary trade theory may be written

$$M_{ij} = \beta_0 P_{ij}^{\beta_1} P_{i*}^{\beta_2} Y_i^{\beta_3} \mu_{ij} \qquad (1)$$

As the relationship expressed in equation 1 is linear when logs are taken on both sides, we may restate that equation in the following form:

$$\log M_{ij} = \log\beta_0 + \beta_1 \log P_{ij} + \beta_2 \log P_{i*} + \beta_3 \log Y_i + \log\mu_{ij} \qquad (2)$$

The parameter on the bilateral price term (β_1) is expected to be negative, while the parameters on the world price term and the income term (β_2 and β_3) are expected to be positive. What this says about an importer's behavior toward a particular exporter (j) is straightforward: other things being equal, as the jth exporter's offered price increases (P_{ij}), the importer will buy less from exporter j. As other exporters' prices rise (P_{i^*}), the importer will buy more from the jth exporter. And as the importer's income rises (Y_i), I expect it will buy more from all exporters, including the jth exporter.

An Extension of the Basic Model

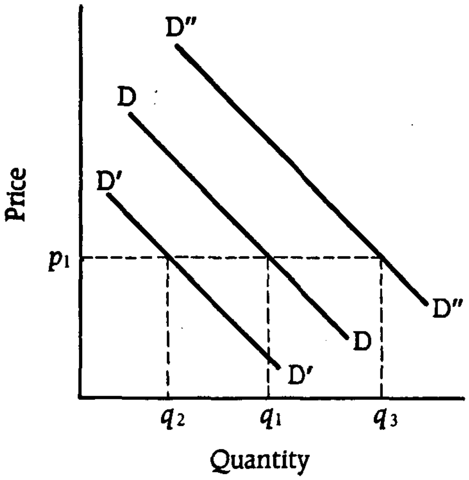
The basic import demand model is not meant to be a complete theory of international trade but serves rather as a cornerstone for many efforts to understand changing international economic relationships. Different economists have added a variety of factors to this model in order to improve its explanatory power or to study the effects of other variables upon trade flows. For example, Hickman and Lau (1973) incorporated price expectations and changes in consumer tastes into the basic model, while Warner and Kreinin (1983) employed it to examine the impact of floating exchange rates upon international trade. Thus I build on the basic model in order to measure the effect of changing international political ties upon bilateral trade.

Extending the Notion of Welfare to Include Political Ties. As I stated, I believe that the flow of trade between two nations will be measurably affected by the general level of friendliness or hostility between them. Other things being equal, greater amity between two nations will mean greater trade between them, while greater hostility will imply less trade. This relationship should hold cross-sectionally (i.e., an importer will purchase more from a friend than a foe) and across time (i.e., imports from a particular trade partner will rise and fall as the political climate between those nations improves or deteriorates). In terms of the basic model, the dependent variable (M_{ij}) remains the same. I am adding a new term to the right-hand side of equation 1—a term that represents the general political relationship between potential trade partners. I denote the term representing the relative cooperativeness of bilateral diplomatic relations D_{ij}. Hence we may rewrite equation 2 as follows:

$$\log M_{ij} = \log\beta_0 + \beta_4 \log D_{ij} + \beta_1 \log P_{ij} + \beta_2 \log P_{i*} + \beta_3 \log Y_i + \log\mu_{ij}. \qquad (3)$$

The addition of this new term carries some important implications. First, it alters the concepts of welfare and utility embodied in equation 1. Specifically, equation 3 contends that an importer's consumption satisfaction is influenced not only by the quantity of goods purchased, but by the general friendliness or hostility of its political relationship with individual exporters. In short, equation 3 expands and complicates the neoclassical notion of welfare. Conceptually, of the various reformulations of the basic model that have been published, the aforementioned study of changing consumer tastes (Hickman

Figure 1. Demand Curves under Different International Political Conditions



DD:	Import demand schedule defined by economic factors alone
D′D′:	Import demand schedule under conditions of greater diplomatic hostility
D″D″:	Import demand schedule under conditions of greater diplomatic cooperation

and Lau 1973) is probably the most similar to my extension. In that study, as well as the work reported here, the factors that shape actor preferences are not limited to the quantity of goods consumed, as they are in the basic model.

How Diplomatic Relations Affect Trade. My model extends the importer's notion of utility in that in addition to traditional economic concerns such as price and quality, an importer's utility will be affected by the wish to express goodwill or solidarity with those whom they identify as friends while shunning or punishing those they perceive as foes. Such considerations can be a way of managing risk; that is, an importer may avoid establishing ties with a seller in an adversary nation, knowing that those ties may be ruptured by one government or the other as part of some foreign policy maneuver. Thus, in cases where price and quality are roughly comparable, the buyer could choose a seller from a friendly nation in order to minimize the possibility of economic disruption. And in a market where good friends as well as strong adversaries may be found, these effects might overwhelm appreciable differences in price and quality. In short, economic and political factors are theoretically coequal in determining import decisions in the sense that neither economic nor political factors alone dominate the importer's decision at all times. The total effect of these factors on a particular trade flow may vary by importer or by trade partner, or they may change over time. The economic factors in the utility function reflect *material* concerns, while the political factors in the utility function reflect traditional *security* concerns: the desire to reward friends, punish adversaries, and minimize risk.

Let us look more closely at the implications for importer behavior of the extended model. Either equation 1 or equation 3 could be portrayed as a multidimensional surface that sets the value of bilateral imports, given values for each independent variable. Either surface would be hard to envision, however, because the surface associated with equation 1 would require four dimensions while equation 2 would require five dimensions. However, if we hold income and prices constant for a moment, then the posited effect of dyadic political relations on trade can be clearly seen.

Figure 1 displays this effect. Each of the three downward-sloping lines in Figure 1 represents a demand schedule, the relationship between the price of a good and the quantity demanded by an importer from a given exporter in the world market. The center line, *DD*, can be viewed as the schedule produced by the basic model discussed above (equation 1). The effect of changing political ties may then be represented as inward and outward shifts of that basic schedule. For example, increased hostility toward a particular exporter would move the basic schedule inward to $D'D'$, while increased cooperativeness toward another trade partner would move the schedule outward to $D''D''$. The key point is this: for any given price (p_1, for example) the quantity demanded by an importer from a particular partner will change as an importer's political relations with particular exporters change (from q_1 toward either q_2 or q_3, depending on the direction of change in diplomatic relations). These shifts in the demand schedule directly reflect the positive relationship I posit between diplomatic friendliness and trade, as well as the negative relationship that I contend exists between political hostility and trade. Thus (in terms of Figure 1) international conflict and cooperation determine to location of the demand curve (DD) and by extension affect the quantity of goods imported.

Considerations of extensions and alternatives to this model are in order here. My professed interest is in the relationships between bilateral commercial ties and the tenor of diplomatic relations. A full specification of these relationships would include export equations as well as import equations, and the effect of commerce on conflict and cooperation would also be specified.[2] I expect that conflict and cooperation affect exports as well as imports and that trade does have an impact on the foreign behavior of nations toward one another. I also expect that later empirical analysis will bear this out. At present, however, data availability dictates a more modest, step-by-step approach in mapping these connections empirically. Thus I have chosen to look now only at imports and only at the effect of diplomacy on commerce. My hope is that this study will contribute to some future specification and estimation of a complete model of the "trade-and-conflict" system.

Arguments can also be made for the inclusion of other variables in my specification of bilateral import demand. Some (such as geographic distance) cannot be included because they do not vary over time and hence would introduce singularity into the data matrix and prevent any estimation. Others (such as exchange rates and differences in the level of development of trade partners) I found not to affect either bilateral trade ties or the impact of conflict and cooperation upon those ties significantly (Pollins 1989).

One modification to the simple model described in equation 3 has been made. I do not expect that all changes in diplomatic ties affect trade flows in the same period. There will often be a contemporaneous effect (as U.S. imports of Soviet goods declined soon after the Soviet invasion of Afghanistan). But I also expect that this effect could persist over time. In this study, the only empirically tractable method of examining both contemporaneous and lagged effects of a change in diplomatic ties on trade is to include these terms directly in the estimating equation. Accordingly, a single lagged term for my measure of diplomatic relations (D_{ij}) has been incorporated. Thus the model that will be estimated is written

$$\begin{aligned}\log M_{ijt} &= \log\beta_0 + \beta_4\log D_{ijt} \\ &\quad + \beta_5\log D_{ijt-1} + \beta_1\log P_{ijt} \\ &\quad + \beta_2\log P_{i\cdot t} + \beta_3\log Y_{it} \\ &\quad + \log\mu_{ijt}. \qquad (4)\end{aligned}$$

My objective is to determine empirically whether bilateral relations of conflict and cooperation influence the trade behavior of contemporary nation-states. Keep in mind that the relationship represented in equation 3 and in Figure 1 concerns trade between two nations. In reality, a nation engages several others in its foreign economic and political relations and changes in any single foreign tie may well be shaped and constrained by relations with third parties, or by systemwide conditions. The model I put forward does not preclude such considerations and indeed can provide a starting point for them. For a particular importer, for example, equation 3 can be used as a template for a series of equations representing several bilateral relationships. Cross-equation restrictions can be included to represent certain systemic characteristics, while an adding-up constraint is imposed to ensure consistency. A full treatment of this model in a systemwide context is beyond the scope of this study, although certain systemic characteristics will be discussed.[3]

Because it is not practical to test the model for a large number of importers, I will assess the scope of its applicability by applying it expressly to three, qualitatively different types of economies: two advanced industrialized capitalist nations, two socialist centrally planned economies, and two developing nations with market economies.

Research Design, Operationalization, and Method

I employ regression analysis to test the model for each of six importing nations. Bilateral import flows are recorded for the years 1955–78 for a set of 25 nations, which includes all 6 importers in this study. Hence, I track imports for each country with each of 24 trade partners.

Research Design: Three Worlds of Development

As stated in the introduction, I selected certain nations in order to ensure that the model would be tested for countries representing different levels of development and modes of production and—as a limited assessment of the effect of trade dependence—to see whether model performance differed between nations with large foreign sectors as opposed to those with small foreign sectors. Many nations would satisfy these conditions.

Six Importers. The United States and the Soviet Union are leaders of the two blocs dominating the postwar world and thus are highly active diplomatically as they pursue myriad, far-reaching interests. They are also the largest trading nations within their respective blocs. The Federal Republic of Germany (FRG) and the German Democratic Republic (GDR) are key members of the West and East blocs. At the same time, the two Germanies continually engage each other in an international relationship of considerable political and economic significance. The FRG is also the second largest importing and exporting nation in the world (second only to the U.S.) while the GDR is second only to the Soviet Union in imports and exports among all centrally planned economies.

Neither Egypt nor India is among the largest trading nations in the developing world, but their diplomatic ties are particularly interesting. Both were pivotal, founding members of the nonaligned movement, which has struggled over 30 years to define a path between the capitalist West and socialist East. Their efforts to travel a middle course have produced interesting turns in their foreign policy and international commercial relations. Egypt, which through the 1960s strengthened its ties with the Soviet Union, broke suddenly with the East in 1974 and realigned itself politically and economically. India has not charted such wide swings in its orientation and has certainly been more evenhanded in its dealings with East and West than either bloc would like. One fact is particularly interesting: looking over a period of several years, India alone among the world's nations holds the United States and the Soviet Union as its top two trade partners. Of course, I claim that the association between India's tightrope diplomacy and its foreign economic ties is no accident, but it remains for my model to demonstrate this association empirically and to assess its magnitude.

Twenty-five Exporters. In addition to the six importers who are the focus of this study, the data set includes five additional advanced market economies (Canada, the United Kingdom, France, Italy, and Japan), two more members of the Council for Mutual Economic Assistance (Poland and Czechoslovakia), five members of the Organization of Petroleum Exporting Nations (Venezuela, Nigeria, Iran, Saudi Arabia, and Indonesia), and seven additional developing countries (Mexico, Brazil, Argentina, South Africa, Turkey, the Peoples' Republic of China, and Pakistan). Clearly, this is not a random sample of nations, but one chosen to include the major actors in the contemporary global political economy, as well as to represent major East-West and North-South cleavages. At present, these 25 nations are responsible for 80% of all goods and services produced in the world, and they ship 67% of all exports (Bremer and Hughes n.d.). They are also responsible for about two-thirds of all cooperation and hostility exchanged, as measured in the "conflict and peace data bank" (Azar 1980). Given such broad-based coverage of every relevant concern, I have no reason to expect any bias in estimation results due to case selection.

Operationalization: Model Indicators

To measure the level of one nation's imports from another (M_{ij}), I gathered annual data on the nominal value of total imports for each dyad over the 24-year period of the study.[4] As my demand equation is stated in real (i.e., constant dollar) terms, it was necessary to deflate these nominal values by price indexes, and I employed the exporter's unit value index for this task.

Next, an indicator of importer income (Y_{ij}) is required. As I treat total imports, rather than imports of specific goods, a measure of aggregate economic activity is most appropriate. For the four market economies included in the study, I employed real gross domestic product (GDP) as the income indicator. For the Soviet Union and German Democratic Republic, data on net material product (NMP) was used.

My model includes two price terms. The first represents the price offered by a particular exporter to an importer (P_{ij}), and I employ the export unit value index for each (j) exporter as the indicator of this price. A second term represents the "world" price seen by an importer (P_{i*}). This world price indicator is constructed by weighting the export price offered by each supplier in the market by the share that each supplier holds in an importer's market. Since these shares may change from year to year, an importer's perceived world price will change not only with movement in exporters' prices but also as the composition of its imports alters.[5]

Finally, operationalization of equation 3 requires an indicator of the current diplomatic relationship between the importer and each trade partner (D_{ij}). I construct such an indicator by employing data contained in the "cooperation and peace data bank" (Azar 1980). The daily events recorded in this data bank are aggregated into annual flows for each dyad, and the information pertaining to the level of conflict or cooperativeness embodied in each act is retained. Thus I have annual observations of the amount of conflict and cooperation between an importer and each of its trade partners. These two diplomatic flows are then combined into a net indicator of relative cooperation, because it is not a particular event or single flow that affects trade, I argue, but the general tone, or climate, of political relations.[6]

Method: Pooled, Cross-section Time-series Analysis

In all statistical work, the efficiency of estimates improves as the number of observations increases. Thus, in studies like this one where cross-sectional, as well as timewise, observations are available, we can get more efficient estimates by pooling the observations (Stimson 1985). The ordinary least squares (OLS) technique may then be applied, and this method is known as "classic pooling." Classic pooling has serious limitations, however, for it is likely that the residuals associated with OLS estimation of a pooled equation will not satisfy the necessary assumptions of randomness. Specifically, the errors may well exhibit autocorrelation as well as nonuniform variance (heteroscedasticity). Statistical inference is impaired by both problems.[7] Because the residuals probably will not conform to the assumptions that make OLS estimation the best technique, an auxiliary model of the residuals is required, a model that will make generalized least squares (GLS) estimation possible.

A wide variety of pooled estimation strategies have been put forward, and some will be more appropriate than others, given the assumptions one wishes to make about the errors. After much experimentation with alternative model specifications and analysis of the residuals produced by OLS estimations of them, I concluded that appreciable heteroscedasticity was introduced by pooling the data and that the individual time series were almost always vitiated by a first order auto-regressive (AR[1]).

Therefore, to produce the results reported in Table 1, I first added 23 cross-sectional dummy variables to the model represented in equation 3 (that is, one for each trade partner save one, to avoid singularity), which controlled for enough cross-sectional variance to yield residuals that exhibited stationarity. Next I used time-series (ARIMA) techniques to diagnose the time-dependent error process within *each* cross section for each of the six importing nations—in almost all cases, first-order autoregression was indicated, and the first-order autocorrelation coefficients (rho) were retained. Finally, I constructed an omega matrix (Ω) from these estimates of the error variance and the first-order autocorrelation coefficient and thus direct Aitken GLS estimation was possible.[8] The resulting parameters are unbiased and the residuals are homoscedastic.

Equation 4 was therefore estimated independently for each of the six importing nations, and our expectations regarding the effects of each independent variable remain clear: the real (constant dollar) imports (M_{ijt}) of nation (i) from nation (j) should be negatively associated with the price offered by the jth exporter (P_{ijt}) and positively associated with the prices offered by exporters other than j (P_{i*t}). The aggregate level of economic activity, or income of the ith importer (Y_{it}), should be positively associated with the level of imports from each partner. Finally, and most importantly for this study, I expect that the contemporaneous and lagged measures of the relative cooperativeness of diplomatic relations between trading nations (D_{ijt} and D_{ijt-1}) will be positively associated with the level of imports by nation i from exporter j.[9]

The results of these estimations are displayed in Table 1. These results show support for the model in general and the relative diplomatic cooperation terms (D_{ijt} and D_{ijt-1}) in particular. Regarding the diplomatic cooperation terms, the estimates carry the expected sign in all cases, and they are statistically significant. I have greater confidence that these estimates are robust because they bear consistently correct signs and are statistically significant across all three worlds of economic development. My confidence in these results is also strengthened by the

Table 1. Estimation Results

Model Terms	USA	FRG	GDR	USSR	Egypt	India
Intercept[a]	-17.96*	-10.10*	-12.76*	-17.63*	-4.118*	5.094*
	(13.09)	(17.69)	(16.20)	(23.91)	(5.496)	(3.290)
D_{ijt}[b]	.013***	.014*	.030**	.036*	.030**	.033**
	(1.896)	(2.807)	(2.452)	(2.853)	(2.270)	(2.030)
D_{ijt-1}[c]	.014**	.014*	.029**	.055*	.037*	.054*
	(2.050)	(2.807)	(2.438)	(4.385)	(2.752)	(3.232)
P_{ij}[d]	.277*	-.647*	-.324**	-.361**	-1.245*	-.684*
	(3.738)	(8.624)	(2.430)	(2.446)	(6.452)	(4.080)
P_{i*}[e]	-.157	.839*	.332	.419***	1.532*	.865*
	(1.520)	(9.203)	(1.507)	(1.844)	(6.381)	(4.110)
Y_i[f]	1.920*	1.242*	1.295*	1.724*	.474*	-.172
	(16.70)	(21.31)	(13.75)	(18.70)	(3.809)	(.956)
$\bar{R}^2$[g]	.857	.888	.872	.861	.822	.796

Note: Coefficients are those for logged values (ln) of model variables, and hence directly interpretable as elasticities; t-values are in parentheses; total number of cases for all six importers is 552.

[a] Variable list excludes 23 dummy variables included in the regression equation.

[b] Relative diplomatic cooperation at time *t*.

[c] Relative diplomatic cooperation at time $t - 1$.

[d] Export price offered by exporter to importer.

[e] "World" price seen by importer.

[f] Indicator of the aggregate economic activity of importer.

[g] (Adjusted) R-squared from GLS estimation.

*Parameter estimates are statistically significant at $p < .01$.
**Parameter estimates are statistically significant at $p < .05$.
***Parameter estimates are statistically significant at $p < .10$.

fact that the model explains about the same amount of the total variance in all six cases; that is, the model fits the data equally well across all six countries.

Discussion

These estimation results are best assessed on two broad dimensions: (1) differences across nations and (2) the effect of diplomatic cooperation on trade flows relative to the impact of other determinants in this model.

Diplomatic Cooperation and Trade in Three Worlds of Development

The effect of diplomatic cooperation on imports conforms to model predictions in all cases, but we do see variation in the size of those parameters across the three worlds of development. Specifically, the contemporaneous effect of diplomatic cooperation (D_{ijt}) on imports is about twice as large in the two centrally planned economies and the two developing economies as it is in the two advanced market economies in this study. The lagged effect (D_{ijt-1}) is two to four times as large when comparing the centrally planned and developing economies on the one hand, and the advanced market economies on the other. These differences point to the substantive significance of the diplomatic cooperation terms.

Recall that economists' models of international trade are based on neoclassical theory and employ all the explanatory variables I have used except the two diplomatic cooperation terms (D_{ijt} and D_{ijt-1}). I argue, therefore, that these two terms represent nonmarket forces affecting trade relations or, more specifically, the extent to which that importer's trade relations are *politically managed.* In other words, I expect that regimes that manage trade relations more directly—nations that frequently intervene in dealings between importers and exporters or operate a state trading monopoly—will exhibit greater sensitivity to international conflict and cooperation in their imports. The foreign economic activity of such nations will be more tightly tied to general foreign policy objectives and behavior, and trade deals will require more direct cooperation with other states in the system.

Another way of viewing the necessity of political management in trade relations may relate to the integration of the state into the world economy. States that are most integrated into the world economy will tend to allow market forces to determine imports and exports, and their trade will be relatively less sensitive to the vagaries of international cooperation and conflict. Even in these cases, however, the theory claims that diplomatic cooperation should be a significant determinant of their imports, and the estimation results bear this out. States that are less integrated into the mainstream of the world economy or that feel their national development objectives are somehow compromised by the operation of world markets will participate more directly in import and export decisions. In these cases, foreign economic behavior will become more integrated into the larger foreign policy of the state and reflect greater sensitivity to the diplomatic and security objectives of the state.

My estimation results suggest that the imports of the Soviet Union, GDR, Egypt, and India are more sensitive to changes in international cooperation and conflict than are those of the United States or the FRG. This is not surprising, given the important role of the central government in the economies of socialist and developing nations. Indeed, any other result would be counterintuitive. The significance of the finding, then, lies in the fact that this model enables us to *measure* differences between nations in the political management of trade ties.

The Importance of Politics: Further Evidence

The 25 countries included in this study are quite varied in foreign policy orientation, level of development, amount of government economic management, and other important dimensions. To date, most studies of trade have focused on a more homogeneous group, namely, the nations of the advanced industrialized West; and these studies demonstrate empirical "success," though they conspicuously ignore political factors (Fair 1979). It is possible, therefore, that the political management of trade relations is significant only when there are great differences between the market and foreign policy orientations of potential trade partners. Perhaps market forces alone determine trade flows among politically homogeneous groups of nations, and the "political management of trade" (as I have called it) is superfluous. To test this proposition, I reestimated my main equation (4) exclusively for the United States' imports from the six other member nations of the Organization of Economic Cooperation and Development (OECD) included in my data set. In short, I tested my model in a trade subsystem where it could be argued that political factors should matter least. The results are displayed in Table 2.

Note that even when attention is focused on nations with similar foreign policy orientations (indeed, most nations in this group are formally allied), I find that imports are significantly influenced

Table 2. Estimation Results: U.S. with OECD Exporters Only

Intercept[a]	D_{ijt}[b]	D_{ijt-1}[c]	P_{ij}[d]	$P_{i\cdot}$[e]	Y_i[f]	$\bar{R}^2$[g]
-30.58* (24.38)	.019** (1.979)	.017*** (1.895)	-1.189* (6.184)	.617* (3.604)	3.035* (28.91)	.968

Note: U.S. with 6 OECD exporters, i.e., Canada, United Kingdom, France, Federal Republic of Germany, Italy, and Japan. Number of cases = 138. Coefficients are those for logged values (ln) of model variables, and hence directly interpretable as elasticies; t-values are in parentheses.

[a] Dummy variables included in regression equations are not reported.

[b] Relative diplomatic cooperation at time *t*.

[c] Relative diplomatic cooperation at time ($t - 1$).

[d] Export price offered by exporter to importer.

[e] "World" price seen by importer.

[f] Indicator of the aggregate economic activity of importer.

[g] (Adjusted) R-squared from GLS estimation.

*Parameter estimates are statistically significant at $p < .01$.
**Parameter estimates are statistically significant at $p < .05$.
***Parameter estimates are statistically significant at $p < .10$.

by international cooperation and conflict. The values of the diplomatic cooperation parameters are virtually the same as those presented in Table 1, and the performance of the price terms and the overall fit with the data are actually improved. Estimating the effect of prices on trade may be complicated by a narrowing or broadening of the set of exporters included, but all indications are that the estimated effect of diplomatic cooperation on imports is significant and robust.

One final test allows us to compare the explanatory power of the customary import demand model (represented by equation 2) against the rival I have constructed (equation 4) directly. It is possible to add a term to an estimating equation and find that the associated parameter is statistically significant without adding much to the overall explanatory power of the equation (Koutsoyiannis 1977, 158–63). In such cases some argue that the additional term should be dropped on grounds of parsimony. It is common to employ an F-test to determine whether the additional term captures a significant amount of the remaining variance. Employing such a test, my estimations of equations 2 and 4 show that the diplomatic cooperation terms do indeed explain a significant proportion of the remaining variance, so that the inclusion of those terms is thus warranted on statistical as well as theoretical grounds.

Conclusion

My immediate objective was to deepen the understanding of international trade patterns by exploring the effect of bilateral diplomatic relations upon imports. To this end, I argued for bridging the theoretical and empirical insights of political science and those of economics and demonstrated my intentions by incorporating a measure of diplomatic relations into a formal model of international trade, a model consistent with current neoclassical theory. I tested this model empirically for six importing nations, representing the three worlds of development. The results confirmed that the relative cooperativeness or hostility of bilateral political ties does indeed affect trade flows between all types of nations, and that the estimates are robust and warrant inclusion on both statistical and theoretical grounds. In short, trade does indeed follow the flag despite the efforts of academics over the past century to consider economics and politics in isolation from one another.

The treatment of importers as rational utility maximizers has shown several benefits of a public choice approach to political economy. Communication between political scientists and economists should be enhanced by the fact that this model is consistent with contemporary neoclassical trade theory. For example, direct side-by-side comparisons were made between the conventional specification of economists and my alternative specification. In addition, the utility function implicit in my model is not limited to notions of price and quality but includes such traditional security concerns as the desire to reward friends, punish adversaries, and minimize risk. This expands the typical conceptualization of "welfare" found in trade theory and thus could help frame new substantive research questions in the field of international political economy in a way that both political scientists and economists could address.

From this beginning, several lines of development are possible. First, the same basic specification developed for this study could be used to study exports. Second, the theoretical and empirical findings in Polachek 1980 and others (discussed above) establish that trade affects bilateral conflict. My findings lend support to the key assumption made by that school and establish that relative cooperation affects trade ties. Therefore, in further exploration of the relationship between commerce and diplomacy, relationships of mutual causation should be considered. Third, domestic interest groups often demand protection. Their activities, of course, often affect commercial policy in particular and trade flows more generally. There is already a significant and growing literature—again, employing the public choice approach—addressing the behavior of such groups. Bridges spanning this literature (concerned with protectionist behavior) and more studies like mine (mapping the political determinants of trade flows) are quite possible. All these are simply the immediate possibilities for future work. They build on already published work and employ the same general approach used here.

I argue for theory building in international politics and economics that does not view such behavior as compartmentalized along the same lines as contemporary academic departments. Existing formal theory, as well as data, measurement, and estimation techniques from both fields, can be fruitfully employed in an effort to regain an understanding of political and economic phenomena that grasps their mutual interrelationships. I believe the results reported here demonstrate the benefits of such an approach.

Notes

Earlier versions of this paper were presented at the Third World Peace Science Congress, 30 May-3 June 1988 in College Park, Maryland and the Annual Convention of the Midwest Political Science Association, 14–16 April 1988 in Chicago. I would like to acknowledge the helpful comments of Tom Milburn, Walter Hill, Henry Brady, Mark Ellyne, Judy McDonald, and Solomon W. Polachek. I also gratefully acknowledge the diligent and highly skilled research assistance of Taehyun Kim. The estimation procedures necessary for this study were difficult and complicated. Mr. Kim solved many problems creatively and contributed greatly to my efforts.

1. In addition, Thursby and Thursby (1984, 124) examine a variety of import demand models, and report superior empirical performance for log-linear formulations. In general, this multiplicative formulation has not been widely used in political science, but it is hardly unknown. Berry and Feldman (1985, 60) describe the elasticity specification in multiple regression, and Hanushek and Jackson (1977, 79) detail advantages held by this approach over some others for estimation purposes.

2. Indeed, I have worked with colleagues on a system of equations to represent these relationships. See Bremer 1987, especially chapters by Pollins and Brecke and by Smith.

3. Readers interested in a systemwide model of import demand representing specific system con-

straints through cross-equation restrictions and an adding-up condition are referred to Pollins and Kirkpatrick 1987. In addition, Kirkpatrick and Pollins (1985) report empirical estimates of such a system for 18 different importing nations. No indicators of diplomatic relations were included in these estimates, however.

4. Specific sources for the indicators described here are, for bilateral imports (M_{ij}), the Direction of Trade data tapes of the International Monetary Fund (IMF); for Council of Mutual Economic Assistance (CMEA) members, data collected by the Vienna Institute for International Economic Comparisons (WIIW); for China, various reports of the Joint Economic Committee of the U.S. Congress and the National Foreign Assessment Center (NFAC); for the income indicator (Y_i), the United Nations' *Yearbook of National Account Statistics;* again, for CMEA members, WIIW data and the same U.S. government sources that provided data for China. Price indicators (P_{ij}, P_{i*}) were constructed from data found in the World Bank's *World Tables,* the *International Financial Statistics* of the IMF, and three United Nations sources—the *UNCTAD Yearbook, Yearbook of International Trade Statistics,* and *Monthly Bulletin of Statistics.* All data used in construction of the indicator of the relative cooperativeness of diplomatic relations (D_{ij}) were taken from the cooperation and peace data bank (Azar 1980).

5. Note that the "world" price is not the same for all importers, simply because different importers have different preferences regarding the goods they wish to purchase (e.g., some will be more interested in petroleum, while others will be particularly interested in high-tech manufactured goods). In other words, the prices seen by consumers vary in part because they shop for different baskets of goods. The method I employ in calculating world price for each importer is a means of taking account of cross-national differences in such preferences.

6. Events data scholars present a number of arguments for combining these flows into a net indicator. Dixon (1983, 831–32) provides a succinct discussion of these points. In this case, combination of the conflictive and cooperative flows was compelling because I incorporate dyadic political relations as a multiplicative term in the model. It simply does not fit my theory to predict zero imports in cases where *either* conflict or cooperation is absent, hence they cannot be represented separately without reformulating the theory. We can write the formula for relative diplomatic cooperation (D_{ij}) as

$$D_{ij} = C_{ij} \cdot \left(\frac{C_{ij}}{C_{ij} + H_{ij}} \right),$$

where C_{ij} is the observed annual flow of cooperation sent by importer (i) to exporter (j), and H_{ij} is the observed annual flow of hostility sent by importer (i) to exporter (j). The form of our measure of aggregate conflict and cooperation is equivalent to the numerator in Cusack's indicator of "hostile intent" (Cusack 1985).

7. If autocorrelation is present, OLS parameter estimates will remain unbiased, but the standard errors of those parameters are likely to be underestimated, thus t-statistics will be falsely inflated (Hibbs 1974, 256–59). Meanwhile, heteroscedastic residuals imply inefficiency in parameter estimates, for the variance of the sampling distribution is greater using OLS than if other estimation approaches are employed (Berk et al. 1979, 391).

8. This is the technique presented by Stimson (1985, 925–29) as "GLS-ARMA" and is described by Stimson as the best pooled estimator under the conditions of my model specification. Chubb (1988, 137n.) partially follows this technique but shuns full application on the grounds that it is "inordinately cumbersome." I agree with Chubb that a great deal of work is involved, especially when one repeats the process independently for six importers and explores alternative specifications. Nonetheless, my results were generated by a full application of the GLS-ARMA technique.

9. Because I derive but one estimate for each parameter in the equation, I must assume that an importer will react identically to a change in prices or diplomatic ties with any exporter in its system. This assumption may be overly strong, but in the absence of theory to guide us, such simplification is not unwarranted. Regarding import demand models in economics, formulations that hold price and income elasticities constant across trade partners are not common, but they exist and are well known. In particular, see Armington 1969 and Hickman and Lau 1973. Thus, concerning the appropriateness of this assumption, only my diplomatic relations term (D_{ij}) is in question, and here I contend that ignorance argues for simplicity. In addition, I conducted exploratory estimations for particular trade subsystems, and these results indicated that this assumption introduces no substantial change in parameters. The theory is presently indifferent on this point, which I leave to later specification and estimation.

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UNION ORGANIZATION IN ADVANCED INDUSTRIAL DEMOCRACIES

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I suggest a new explanation of cross-national differences in unionization rates: the size of the labor force. Size matters because the gains unions are able to achieve in collective bargaining depend on the proportion of substitutable workers who are organized, while the costs of organizing depend in part on the absolute number to be recruited. The comparison of the costs and benefits of organizing new workers yields the conclusion that unions in larger labor markets will accept lower levels of unionization. Statistical analysis of cross-national differences in unionization rates among advanced industrial societies in the late 1970s indicates that the size of the labor force and the cumulative participation of leftist parties in government explain most of the variance.

Few features of economic, social, or political life in industrialized democracies differ as much as the relative size of the trade union movement. The current density of union membership in the labor force ranges over almost the entire spectrum from above 90% in Sweden to under 20% in the United States (Goldfield 1987, 16). The level of unionization varies far more than such other characteristics of the labor force as the sectoral distribution of workers, the share of wages in GNP, rates of unemployment, or even the size of the public sector. Unionization rates vary more than such other forms of popular mobilization as electoral turnout or the share of the vote received by parties bearing communist, socialist, social democratic, or labor labels.

The economic effects of high levels of unionization are ambiguous. Unions that are large relative to the economy may simultaneously have more power in the labor market and more of an incentive to moderate their wage demands. A union that covers only a small fraction of an industry's work force, for example, can gain wage increases partly at the expense of employment among nonunion members, provided that union members have specialized skills not readily available elsewhere. In contrast, an industrial union covering the entire work force would be concerned with employment in all job categories. Bigger unions are not necessarily more militant unions (Cameron 1984; Olson 1982, chap. 4).

The political consequences of high levels of unionization are more straightforward. There is general agreement that, other things being equal, union movements representing a large share of voters are better able to influence policy. Union density is often used, either alone or in combination with measures of union centralization and unity, as an indicator of union power in cross-national comparisons of employment and welfare policy (Cameron 1984; Hicks 1988; Hicks and

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Swank 1984a, 1984b; Korpi 1983; Korpi and Shalev 1979, 1980; Stephens 1980). In addition, high levels of unionization are positively associated with the electoral support received by Socialist, Social Democratic or Labor parties (Korpi 1983; Przeworski and Sprague 1986; Shalev and Korpi 1980; Stephens 1980).[1] Union density is an important—albeit not the only—determinant of union strength in the political arena as well as the market.

Table 1. Union Membership as a Percentage of Potential Union Members in Advanced Industrial Democracies (late 1970s)

Country (Year)	Union Density
Sweden (1979)	82.4[a]
Israel (1979)	80.0[b]
Iceland (1975)	74.3
Finland (1980)	73.3
Belgium (1977)	71.9
Denmark (1980)	69.8
Ireland (1978)	68.1
Austria (1977)	65.6
New Zealand (1979)	59.4
Norway (1979)	58.9
Australia (1979)	51.4
Italy (1978)	50.6
United Kingdom (1976)	48.0
Germany (1979)	39.6
Netherlands (1979)	37.7
Switzerland (1979)	35.4
Canada (1980)	31.2
Japan (1979)	31.0
France (1979)	28.2
United States (1978)	24.5

Note: Potential union members are defined to be all wage and salary earners plus the unemployed. Data sources are listed in Appendix B. Union density in Luxembourg in 1978 was over 60% (Coldrick and Jones 1979). How much over is impossible to say as membership data for two of the three large labor organizations outside the main confederations are unavailable.

[a] Bain and Price (1980, 143) report a significantly higher unionization rate for Sweden—92.9—in 1977. Bain and Price note that their figure for Sweden includes retired union members which they estimate to comprise between 3% and 8% of the total (139).

[b] Dividing membership in the Histadrut by total wage and salary earners plus the unemployed results in a unionization rate of around 150%. Coldrick and Jones (1979, 1179) estimate that 85% of wage earners are members of the Histadrut while the estimate of the U.S. Department of Labor (1980, 145) is between 75% and 80%. The figure of 80% seems a suitable compromise.

Table 1 presents union membership as a percentage of potential members (defined as all wage and salary earners plus the unemployed) in advanced industrial societies in the late 1970s.[2] The distribution of unionization rates was surprisingly uniform between the extremes of Sweden and Israel on the one hand and the United States on the other. In particular, while the United States was at the low end of the spectrum, it was hardly in a class of its own. The unionization rates in France, Japan, and Canada were only slightly higher.

Cross-national differences in union density have increased since the mid-1950s, with unionization steadily declining in countries at the low end of the spectrum, such as the United States and Japan, while continually growing in countries at the high end, such as Sweden and Denmark (Bain and Price 1980; Troy and Sheflin 1985). Yet the relative positions of countries for which time series data exist have not changed much in the past three decades. In the postwar period the variation of union density among Western democracies far surpasses the variation over time within countries for which data is available.[3]

While there has been little work that attempts to explain cross-national differences in union density, there is a large and interesting empirical literature on the dynamics of union growth.[4] In fact Samuel Gompers was an early theorist. In 1904 he presented his view as follows: "From the formation of the first bona fide trade union movement in modern times, it has grown with each era of industrial activity and receded to some degree with each industrial depression, but with each recurring period of depression it has receded to a lesser degree than its immediate predecessors" (cited in Lorwin 1933, 233). This business cycle hypothesis was taken up by Commons and his associates at the University of Wisconsin (1918) and seemingly confirmed in empirical studies by Barnett (1916) and Weyworth (1917). During periods of high demand for goods and tight labor markets, the reasoning went, rising prices eroded real wages, prompting workers to join unions in defense of their real income while the greater effectiveness of strikes lowered the resistance of employers.

In the 1920s, however, the business cycle hypothesis suffered major damage by the failure of unions to grow. When unions began to rebound in the depths of the Great Depression, the procyclical hypothesis appeared decisively refuted. Dunlop (1949, 191) inaugurated a new phase of the discussion with an argument that union growth occurred either in periods of war when demand was particularly high and labor was in particularly short supply or in periods of "fundamental unrest," when the organization of unions "represented a basic dissatisfaction with the performance of the economic system and the society in general." In the early 1950s Bernstein (1954) called for a "pluralist" conception of union growth allowing for different causes of union growth in different historical periods. In fact, Bernstein gave greatest emphasis to the impact of legislation. Shister (1953), similarly doubtful about the explanatory power of the business cycle hypothesis, argued that union leadership was decisive.

Ashenfelter and Pencavel (1969) initiated the third and final phase of the discussion by successfully fitting a relatively simple equation to union membership in the United States between 1904 and 1960. An econometric literature soon developed with scholars estimating time series models of union membership data from the turn of the century to the recent past in a half dozen countries (Bain and Elsheikh 1976; Hines 1964, 1969; Sharpe 1971; Swindinsky 1974). The thrust of the econometric results can be summarized in three findings: (1) the rehabilitation of the business cycle hypothesis—business cycle variables, primarily price inflation and secondarily unemployment and wage inflation, explain most of the variance of annual changes in union membership in all countries; (2) the saturation effect—as union density increases, union growth slows down; (3) the relative unimportance of political variables—political variables, by and large, did not have a discernable effect on union growth. For example, Bain and Elsheikh (1976) fit a business cycle model of union growth in four countries: the United Kingdom, United States, Australia, and Sweden. Only in Australia prior to 1913 and in the United States from 1937–47 did the fit of the model improve with the addition of a political dummy variable. Other political variables had no impact.

The new business cycle models have not escaped criticism. Moore and Pearce (1978); Sheflin, Troy, and Koeller (1983); and Fiorito (1982) have shown that the models lack stability: when estimated for the postwar period alone, many of the coefficients on the key cyclical variables lose significance and even change sign. Moreover, the connection between the statistical models of union growth and economic theory is vague. Unions tend to be portrayed as defending whatever happens to be the income of their current members rather than maximizing some objective function under the constraint of the response of employers or the government. Most importantly for my subject, the large national differences in union density remain a mystery when union growth is primarily explained by cyclical movements in prices and unemployment.

The prevalent explanations of the variance of unionization rates, whether in time series or cross-sectional studies, generally focus on circumstances that either increase the propensity of unorganized workers to join unions or reduce the cost of unionization for employers (Hirsch and Addison 1986, chap. 3). In contrast, I concentrate on the unions' decision to allocate resources to the recruitment of new members. From the traditional perspective, union growth occurs when workers organize unions. But it is equally true that union growth occurs when unions organize workers. In my analysis unions are the central actors.

That the level of unionization is the subject of strategic decisions made by union, and let $\mu = m/n$ be the proportion comparative politics.[5] Craft unions—unions that restrict entry—are not very popular among those who are not members, scholars included. The restrictive practices of the American Federation of Labor are attributed to the "nativism" or the "racism" of U.S. workers. In Great Britain the "labor aristocracy" is charged not only with betraying their fellow class comrades but with sharing the spoils of imperialist exploitation. On the other hand, unions that are committed to organizing "every man who earns his livelihood either by his brain or his muscle" (in the words of Big Bill Haywood at the founding convention of the Industrial Workers of the World, quoted in Weinstein 1975, 9) are regarded as examples of selfless, if impractical, dedication to the ideals of class solidarity. In contrast to both views, unions are regarded here as organizations seeking to do the best for their members under different circumstances, whether at a particular time they aggressively mount organizing drives or let membership decline through attrition and retirement.

In the next section bargaining theory is used to model the impact of union density on the outcome of collective bargaining. The model is expanded in the third section to derive the level of unionization that maximizes the benefits received by union members. The central conclusion of the analysis—that the optimal union density is a negative function of the size of the potential union membership—is tested in the fourth section in a cross-sectional analysis of union density in advanced industrial societies where unions have been free to organize since World War II. In the concluding section the argument is generalized to encompass political objectives on the part of unions as well as organizations other than trade unions. Proofs of the propositions in the text have been collected in Appendix A.

A Model of Collective Bargaining and Union Density

Without a capacity to disrupt the supply of labor to employers, unions would be powerless participants in collective bargaining. As Wilkinson and Burkitt have written, "The ultimate sanction of union power is the ability to undertake strike action and so impose losses upon the employers of an industry" (1973, 113). The cost that unions can impose depends on the solidarity not only of their members but of all who could take their place. Unions seek to recruit new members because the presence of nonunion workers who can perform the same tasks as union members limits the gains unions can obtain at the bargaining table.

The impact of union density on the outcome of collective bargaining can be illustrated by introducing the level of unionization in a model of collective bargaining. Both unions and firms are assumed to be perfect agents of their (risk-neutral) constituents: firms maximize profits and unions maximize the expected income of union members. Let w be workers' income if they work and receive the union wage, and r their income if they are laid off or if negotiations break down and a strike ensues.[6] In addition, let the probability of remaining employed at a union job in the absence of a strike be denoted $\Theta(L)$, where L represents the firms' demand for labor. Workers' expected income can be written, then, as

$$u = \begin{cases} \Theta(L)w + [1 - \Theta(L)]r & \text{if there is no strike} \\ r & \text{if a strike occurs.} \end{cases} \tag{1}$$

The assumption in equation 1 that workers are risk-neutral—or maximize their expected income rather than their expected utility—simplifies the analysis without altering the conclusions regarding the optimal union size.[7]

Unless there is a strike, firms' profits, π, are defined to be the difference between their revenues, $R(L)$, and the wage bill wL. As is conventional, the marginal revenue product of labor is assumed to be positive and diminishing as employment increases with other inputs held constant, that is, $\partial R/\partial L > 0$ and $\partial^2 R/\partial L^2 < 0$. In the event of a strike, however, let firms' profits be denoted S. (The determinants of S will be elaborated shortly.) Thus, profits can be written as

$$\pi = \begin{cases} R(L) - wL & \text{if there is no strike} \\ S & \text{if a strike occurs.} \end{cases} \tag{2}$$

The cost of a strike to firms $[R(L) - wL - S]$ depends on the level of production that firms can maintain without the striking workers. In previous work it has generally been assumed that profits go to zero when a strike occurs (McDonald and Solow 1981). Yet the extent that a firm will have to reduce production depends, among other things, on the proportion of the firms' plants or workers that are organized and the availability and willingness of other workers to take the place of those on strike. Note that strike effectiveness depends on the proportion of the work force that refuses to work rather than the absolute number of strikers. Let M be the number of union members in the local bargaining unit, let $\Sigma M = m$ be the total membership of the union, let n be the total number of available workers with the requisite skills including both those in the union and those outside the union, and let $\mu = m/n$ be the proportion unionized or the union density. Then the profits received by firms during strikes can be written:

$$S = S(\mu, x) \text{ with } \partial S/\partial \mu < 0 \text{ and } \partial^2 S/\partial \mu^2 > 0. \tag{3}$$

That $\partial S/\partial \mu < 0$ indicates that profits during a strike are a declining function of the proportion of the available workers who will refuse to work. It is assumed that $\partial^2 S/\partial \mu^2 > 0$ since, at least at high levels of unionization, further increases in union density must have a diminishing marginal impact on the union's power to reduce production (Block 1980). The variable x is a vector representing all of the factors besides the level of unionization that affect the profits or losses firms would sustain during a strike: the level of the firm's inventories or the demand for the firm's output, the support offered by an employers' association, the likely reaction of the government, legislation covering industrial conflict, etc.

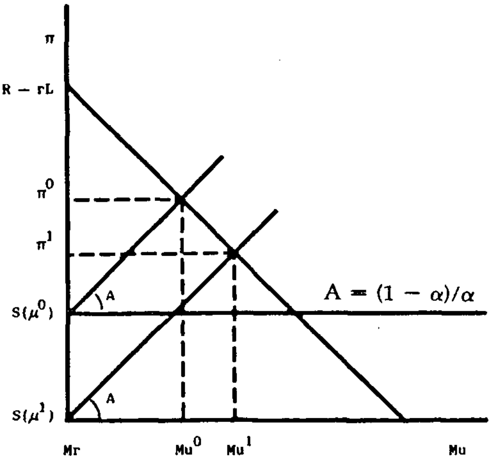
Multiple solutions have been proposed to the bargaining problem. In one frequently studied case, however, where the number of union members in the bargaining unit exceeds or equals the demand for labor and all union members are equally likely to be laid off, most bargaining solutions coincide. If $\Theta(L) = L/M$ with $L \leq M$, the Nash, the Kalai-Smorodinsky, the Maschler-Perles, the proportional, and the Rubinstein solutions all yield identical results.[8] Moreover, this assumption of equal probability of layoffs is less outlandish than it appears. Lazear (1983) demonstrated that if workers are laid off strictly by seniority, unions that maximize workers' lifetime earnings will act as if to maximize $(L/M)w + [1 - (L/M)]r$ every period.

An intuitive derivation of the general bargaining solution when $\Theta = L/M$ and $L \leq M$ proceeds by dividing the bargaining problem into two steps.[9] First, union members and the firm maximize the total income both sides would receive with a contract, $\pi + Mu = [R(L) - wL] + [wL + r(M - L)] = [R(L) - rL + rM]$, by agreeing to the level of employment given by the condition $\partial R/\partial L = r$. In an efficient bargain with risk-neutral workers, the level of employment is determined by the requirement that the marginal revenue product of labor equal labor's opportunity cost (Menil 1971).[10] Second, the aggregate gains to union members from an agreement, $(w - r)L$, is set equal to some share, say $\alpha (0 < \alpha < 1)$, of the gains received by both parties $(R - wL - S) + (w - r)L = (R - S - rL)$, which yields

$$w = \alpha[(R/L) - (S/L)] + (1 - \alpha)r \quad (4)$$

as the expression for union wages. Thus the wages of union members depend on the bargaining parameter, α; workers' productivity, (R/L); and the income of the firm and of workers should the two sides fail to agree, S and r. If union members could be replaced quickly with other workers at the reservation wage, the firm's profit during a strike would be $S = R - rL$ and equation 4 would reduce to $w = r$. In noncooperative bargaining theory, the parameter α is determined by the relative impatience of the two sides. The more workers discount the future relative to firms, the lower is α.[11] In cooperative bargaining theory α is either set equal to one-half by an axiom of symmetry or left undetermined.

Figure 1. The Impact of Changes in Strike Effectiveness on the Bargaining Solution



The impact of S on the bargaining outcome can be seen clearly by writing the expressions for the expected income of workers and their employers upon the conclusion of a labor agreement. Substituting equation 4 into equations 1 and 2, one obtains

$$\begin{aligned} Mu &= \alpha[R(L) - S(\mu,x) - rL] + rM \\ \pi &= (1 - \alpha)[R(L) - rL] + \alpha S(\mu,x). \quad (5) \end{aligned}$$

As union density increases, a strike becomes more threatening (S falls) and union members obtain more of the joint surplus.[12]

The bargaining model is illustrated in Figure 1. The line with slope of -1 represents the set of feasible bargains. The solutions are the points of intersection of the bargaining frontier and the rays from the disagreement points with slopes of $(1 - \alpha)/\alpha$. As union density rises from μ^0 to μ^1, the disagreement point falls from $S(\mu^0)$ to $S(\mu^1)$, thereby raising the aggregate expected income of union members from Mu^0 to Mu^1 and reducing the profit of unionized firms from π^0 to π^1.

The Optimal Rate of Unionization from the Union's Perspective

If the story told in Figure 1 were complete, unions would always want to increase the level of unionization. But recruitment of new members is not free. Organizers must be paid. At times strikes must be called. According to estimates by Voos, the organizing cost of adding one additional union member in the United States ranged from \$580 to \$1,570 (1980 dollars) in the period 1964–77 (1984a, 44). Thus, if marginal costs were constant, unions would have had to spend an additional \$100–300 million (1980 dollars) per year between 1964 and 1977 to maintain union density at 1964 levels. This would have meant an increase of 50%–150% over what unions actually spent.[13] Labor legislation and the manner in which the legislation is enforced probably make union organizing particularly expensive in the United States (Freeman and Medoff 1984, chap. 15; Goldfield 1987, chap. 9; Rogers 1985). Unfortunately, comparative figures on organizing costs are unavailable. But unions in all countries that seek to recruit new members must devote resources to organizing.

Organizing costs depend on many factors: occupation (blue-collar workers are generally more highly organized than white-collar workers), plant size (large plants are more highly organized than small plants), and the threat of unemployment, as well as the political support or hostility toward union growth. In addition, organizing costs depend on the number of workers being organized.

To put these considerations in mathematical form, let the aggregate expenditures of the union on recruitment be written

$$C = C(z,y), \text{ with } \partial C/\partial z > 0 \text{ and } \partial^2 C/\partial z^2 \geq 0, \qquad (6)$$

where $z \equiv dm/dt \geq 0$ is the number of new members who are successfully recruited and y is a vector of all other factors—such as plant size, the business cycle, labor legislation, or employer resistance—that affect the total and marginal costs of recruitment.[14] The first inequality —$\partial C/\partial z > 0$—states that costs increase as the number of workers being organized increases (Voos 1983). Otherwise, unions could achieve a larger membership by reducing their organizing efforts. The second inequality—$\partial^2 C/\partial z^2 \geq 0$—states that marginal costs of organizing do not fall. This last assumption is necessary, at least in the neighborhood of the solution, for the existence of an optimum. If marginal costs are decreasing, unions can always do better by either expanding or contracting the scale of their organizing effort. Note that constant or increasing marginal costs does not mean that the cost per new member $C(z,y)/z$ is necessarily rising. If there are fixed costs in organizing, increasing marginal costs for all $z \geq 0$ imply a very plausible cost curve with average costs that initially fall and eventually rise as the scale of organizing increases.

Union members' expected income rises as union density grows. But union members must each pay organizing costs of $C(z,y)/m$ in union dues. From equation 5 the expected income of union members before union dues is $u = (\alpha/M)(R - S - rL) + r$. Therefore, the net income of union members in each time period is $u - (C/m) = (\alpha/M)(R - S - rL) + r - (C/m)$, which can be rewritten as $P - QS - (C/m)$, where $P \equiv [(\alpha/M)(R - rL) + r]$ and $Q \equiv \alpha/M$. Thus, a union that faithfully maximizes the present value of its members' net income with a discount rate of ϱ would choose the level of organizing activity $z(t)$ that maximizes the integral:[15]

$$\int_0^\infty e^{-\varrho t} \left[-QS[\mu(t),x] - \frac{C[z(t),y]}{m(t)}\right] dt, \qquad (7)$$

where $\mu(t) \equiv m(t)/n$ and $z(t) \equiv dm/dt$.

The necessary conditions for an optimum are shown in the Appendix to define a path of $\mu(t)$ that converges to a stationary level of unionization, denoted μ^* (provided $\mu(0) < \mu^*$ and $0 < \mu(t) < 1$ along the optimal path). This target union density depends on the union's rate of discount (ϱ), factors affecting the effectiveness of strikes (x), factors affecting the costs of organizing (y), and the size of the potential membership (n). The results can be summarized in the following four propositions. (All proofs are presented in Appendix A.)

PROPOSITION 1. *As the union's discount rate* ϱ *increases, the target union density* μ^* *declines, or* $\partial\mu^*/\partial\varrho < 0$.

This is an intuitive result. Organizing drives are like an investment. The costs of recruiting new members are borne in the present while the benefits come in future negotiating rounds. The less union members care about the future, the higher the discount rate and the less unions invest in increasing the size of the union. This fits the common observation that high rates of turnover are associated with low rates of unionization.[16] For the same reason, unions with older members would devote fewer resources to organizing than unions with a young membership.

PROPOSITION 2. *Any change that increases marginal strike effectiveness increases the target union density, or* $\partial\mu^*/\partial x > 0$ *if and only if* $\partial^2 S/\partial\mu\partial x < 0$.

Anything that increases the likely cost of a strike to the firm such as high levels of demand for the firms' output will increase the optimal unionization rate.[17] Defensive actions by employers' associations like the establishment of strike insurance funds may also propel unions to devote more resources to organizing by increasing the necessity of attaining high levels of density in order to strike effectively.

PROPOSITION 3. *A change that increases total and marginal costs of recruiting new members in equal proportions will reduce the target union density, or* $\partial\mu^*/\partial y < 0$ *if* $C(z,y) = yc(z)$, *with* $y > 0$.

Proposition 3 is the counterpart of proposition 2. Changes that increase the cost of recruiting additional members, such as right-to-work legislation, lower the target unionization rate.[18] Both propositions 2 and 3 can be readily understood with the analogy of unionizing drives as an investment. If the rate of return rises, the optimal level of investment will increase. If the cost of investing rises, the optimal level of investment falls. The focus here on the unions' preferred level of unionization is not in conflict with the usual attention given to political support for unions, the resistance of employers, and the sentiment of nonunion workers. The level of economic activity and the political environment affect the costs and benefits of organizing, in part, by influencing the willingness of nonunion members to join and employers to resist.

PROPOSITION 4. *The target level of unionization declines as the size of the labor force increases, or* $\partial\mu^*/\partial n < 0$.

The union's optimal size may either rise or fall as the number of potential members increases. The effect of an increased number of potential members on the union's optimal density, however, is unambiguously negative. The benefits from an expansion of the union membership depend on the proportion of the relevant work force being organized. The costs, in contrast, depend in part on the absolute number of workers to be organized. Unions in large labor markets must pay a higher price than unions in small labor markets to achieve the same union density. Since the price is higher, unions in large labor markets purchase less. Even though there are more members to share the costs of organizing as the membership grows, unions maximize the net gains from organizing by organizing a smaller share of potential members when the number of potential members is large.

Many of the assumptions of the model can be easily relaxed or altered. Expected income maximization can be changed to expected utility maximization. The infinite time horizon can be replaced by a two-period model where the costs of organizing are paid in period 1 while the benefits of higher levels of unionization are received in period 2. The saturation effect noted in the time series literature can be incorporated by writing organizing costs as $C = C(z,y,\mu)$ with $\partial C/\partial\mu \geq 0$, $\partial^2 C/\partial\mu^2 \geq 0$, and $\partial^2 C/\partial z \partial\mu \geq 0$. Finally, greater realism can be added by including changes in union membership over time that are independent of organizing efforts according to the formula: $dm/dt = z(t) - \delta m(t)$, where $\delta m(t)$ represents the net effect of membership losses from retirements, quits, and layoffs and membership gains from increases in employment in union shops. Most of these modifications entail increases in algebraic complexity; none alter the results.

Empirical Results

The analytical results are consistent with most of the standard explanations of variation in union density. The level of demand, the political support, plant size, occupational structure, and so on can all be understood as altering either strike effectiveness or the costs of organizing or both. What the model adds is a new potential explanation of differences in union density: the size of the labor pool. The size of the relevant work force from the point of view of a single union is the number of workers who are actual or potential substitutes in production. However, holding industry or occupation constant, the size of the relevant work force from the point of view of individual unions is likely to be related to the size of the work force of the country as a whole. Moreover, the size of the aggregate labor force is something that differs dramatically among advanced industrialized countries. The number of wage and salary and unemployed workers in the United States is 24 times the number in Sweden and over 1,000 times the number in Iceland. Perhaps the sheer number of potential union members explains an important part of cross-national variation in union density.

Dependent Variable and Sample

The dependent variable is total union membership in the late 1970s divided by the number of wage and salary earners plus the unemployed. The sample consists of the 20 advanced industrial societies that have been democracies since the end of the Second World War (with the exception of Luxembourg—for lack of adequate data). A theory premised on the choices of unions is applicable only where unions are independent of the government and can organize without fear of repression. In addition, instantaneous jumps to the optimal stationary level of union density are generally not optimal. Therefore, the unions' optimal level of density at any point in time depends on current levels of unionization as well as their target level of unionization. However, in countries where unions have been free to organize over a long period of time, the target level of union density might provide a good approximation for the actual levels attained. The countries included in the sample and their union density are listed in Table 1.

Independent Variables

Size of the Labor Force. The measure of size used is the natural log of the number of wage and salary earners plus the unemployed. Using the log of the potential membership implies that the percentage increase, rather than the absolute increase, matters for union density.[19]

Left Governments. Both common sense and the analytical results indicate that cross-national differences in the legal and political environment might have a significant impact on unionization rates. Government support that affects the cost of organizing new workers or the ability of unions to strike alters the unions' optimal level of unionization. Leftist governments are potentially important for union density for two reasons. First, leftist parties are particularly sympathetic to union views on labor legislation. Thus leftist party participation is as good a proxy as any available for the extent to which laws protect union organizing. Second, the implementation of labor law and political support more generally can be just as important as the laws themselves for union organizing. Therefore, I included an index of cumulative leftist party participation in government from 1919 to the year of the union density figure in the analysis.[20]

Yet just as leftist governments may enable or encourage unions to increase union membership, more encompassing unions may increase the votes received by leftist parties and the probability that leftist parties will govern. In many countries, union organizing was initially closely associated with and supportive of the political mobilization of workers by social democratic parties (Kassalow 1980; Przeworski and Sprague 1986; Shalev and Korpi 1980). Since leftist governments and union density are likely to be simultaneously determined, the method of two-stage standard least squares (SLS) was employed with a dummy variable for union centralization as the instrument.[21]

Trade Dependence. Another explanation of cross-national differences in union density, associated most prominently with the work of Cameron (1978) and Katzenstein (1985), emphasizes the impact of trade dependence. According to Katzenstein's argument, perceptions of vulnerability to the vicissitudes of international markets induce the leadership of both business and unions in trade-dependent economies to seek an institutional framework within which compromises can be achieved amicably and conflict avoided. Among other things, the institutional framework that Katzenstein associates with trade dependence entails high levels of union density (1985, 32–33, 91). Thus merchandise exports as a proportion of GDP was included in the analysis as a measure of trade dependence.

Proportion of the Work Force in Mining, Manufacturing, Utilities, and Construction ("Industry"). Countries differ in the mix of industries that make up the national economy. Since unionization rates vary by industry within the same country, it is possible that much of the cross-national variation in aggregate union density reflects cross-national variation in the distribution of workers among industries rather than differences in unionization rates, holding industry characteristics constant. Ingham (1974) and Cameron (1978), among others, have argued that differences in industrial structure explain much of the cross-national variation of the organization of the union movement, possibly including the level of unionization. It is doubtful that cross-national differences in average firm size are important: data collected by Wright (1985, 209) in Sweden and the United States on private sector employment by size of firm reveal virtually identical distributions for the two countries.

Nevertheless, important differences in industrial structure among countries may have an impact on the level of unionization. Cross-sectional studies of union density at the state or metropolitan level in the United States have found that unionization is significantly higher in areas with a large proportion of the work force in manufacturing, transportation, communications, and utilities and a small proportion in agriculture (Hirsch 1980). Therefore, the proportion of the national work force in the more heavily unionized sectors of mining, manufacturing, utilities, and construction was added as another independent variable.

Proportion of the Work Force in Blue-collar Occupations. Blue-collar workers are generally more highly organized than white-collar workers. Like the proportion of workers in manufacturing, the proportion of workers in blue-collar occupations is significantly related to differences in unionization levels within the United States (Hirsch 1980; Hirsch and Addison 1986, chap. 3).[22] Accordingly the number of production workers, transport and equipment operators, and laborers as a proportion of the economically active population was also used in the statistical analysis, although the data on the distribution of the work force by occupation (collected by the International Labour Office from national censuses and labor force surveys) is not very reliable. No occupational data could be found for Belgium, Italy, or Iceland.

Results

Table 2 displays the results of the statistical analysis. Both ordinary least squares

Table 2. Cross-Sectional Analysis of Union Density in Advanced Industrial Societies (late 1970s)

	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Constant	98.6	90.4	77.7	70.3	83.4	75.5
	(7.04)	(4.97)	(2.73)	(2.18)	(2.27)	(1.61)
Size[a]	-6.52	-5.92	-5.98	-5.60	-6.35	-5.84
	(4.32)	(3.37)	(3.23)	(2.78)	(3.17)	(2.11)
Left[b]	.27	.36	.28	.33	.30	.34
	(3.73)	(2.62)	(3.68)	(2.66)	(3.82)	(2.05)
Trade[c]	—	—	.15	.16	—	—
			(0.59)	(0.62)		
Industry[d]	—	—	.34	.40	—	—
			(0.59)	(0.67)		
Blue-collar[e]	—	—	—	—	.33	.39
					(0.39)	(0.44)
R^2	.74	.72	.75	.75	.80	.79
Number of cases	20	20	20	20	17	17

Note: The dependent variable is union membership as a proportion of potential membership. Numbers in the parentheses are t-statistics (asymptotic t-statistics in the case of 2SLS). See Tables 1 and 3 and Appendix B for data and sources.

[a] Natural log of potential membership.

[b] Wilensky's (1981) index of left party participation in government cumulated from 1919 to the year listed in Table 1.

[c] Merchandise exports as a share of GDP.

[d] Employment in mining, manufacturing, utilities, and construction as a proportion of civilian employment.

[e] Production workers, transport and equipment operators, and laborers as a proportion of the economically active population.

Table 3. Potential Union Membership, Cumulative Left Governments, Estimated, and Actual Union Density

Country (Year)	Potential Membership (in Thousands)[a]	Index of Left Governments[b]	Estimated Density[c]	Actual Density
Sweden (1979)	3,931	111.84	81.1	82.4
Israel (1979)	997	73.17	75.5	80.0
Iceland (1975)	81	17.25	70.5	74.3
Finland (1980)	2,034	59.33	66.3	73.3
Belgium (1977)	3,348	43.25	57.7	71.9
Denmark (1980)	2,225	90.24	76.8	69.6
Ireland (1978)	886	0.00	50.2	68.1
Austria (1977)	2,469	48.67	61.4	65.6
New Zealand (1979)	1,050	60.00	70.5	59.4
Norway (1979)	1,657	83.08	76.0	58.9
Australia (1979)	5,436	33.75	51.4	51.4
Italy (1978)	15,819	0.00	33.1	50.6
United Kingdom (1976)	25,757	43.67	45.7	48.0
Germany (1979)	23,003	35.33	43.4	39.6
Netherlands (1979)	4,509	31.50	51.7	37.7
Switzerland (1979)	2,460	11.87	48.3	35.4
Canada (1980)	10,516	0.00	35.5	31.2
Japan (1979)	39,930	1.92	28.3	31.0
France (1979)	18,846	8.67	35.1	28.2
United States (1978)	92,899	0.00	22.6	24.5

Note: Data sources are listed in Appendix B.

[a] All wage and salary earners plus the unemployed.

[b] Wilensky's (1981) index of cumulative left governments from 1919 to the year in parentheses.

[c] Predicted values from equation 2 in Table 2.

(OLS) and 2SLS estimates (with leftist government and union density as endogenous variables) are reported. The difference in the results obtained with the two methods is not large in this case. The size of the work force and the cumulative government of leftist parties are both strongly associated with cross-national differences in unionization. The coefficient on the log of potential membership (size) is consistently negative and significant at conventional levels of significance. A coefficient of −6.0 indicates that a doubling of the potential union membership would reduce union density by ln(2)(6.0) = (.69)(6.0) ≈ 4.2 percentage points. The coefficient on leftist party participation in government (left) is positive and also highly significant. Its size indicates that one year of a social democratic majority government (three points in Wilensky's index) increases union density by approximately one percentage point. Together, size and leftist party participation explain about 70% of the variance.

In contrast, none of the other variables proved to be statistically significant in any combination. Adding either the proportion of the work force in industry or trade dependence to an equation with the proportion of blue-collar workers changed the sign of their coefficients from positive to negative (not shown). After controlling for the size of the potential membership and the cumulative impact of leftist governments, neither trade dependence nor highly aggregated measures of industry and occupational characteristics contribute much to the explanation of cross-national variation in unionization levels.

The OLS and 2SLS estimates do well in

accounting for the variation of union density by the size of the labor force and the cumulative participation of leftist parties in government. None of the predicted values (presented in Table 3) fall outside the permissible bounds of zero and one. Yet since the dependent variable is bounded on both ends, an unbounded linear relationship cannot be literally correct. In addition, the errors cannot be normally distributed. Both difficulties can be overcome with a logistic transformation of union density $\lambda = \ln[\mu/(1 - \mu)]$. Repeating the regressions of Table 2 with λ as the dependent variable (not shown) produced similar results with virtually identical t-statistics on all coefficients.

The insignificance of cross-national differences in the composition of economic activity in Table 2 may be an artifact of the high level of aggregation. More disaggregated comparisons can be done where data on union membership at the industry level is available. Bain and Price (1980, 38, 43–78, 89) have comparable union membership and labor force data for 15 sectors of the economy in the United States and Great Britain in 1974.[23] Unfortunately, the industry-level union membership data for the United States include Canadian members, who constitute 7%–8% of the total (100). One can calculate, however, what the British rate of unionization would be if Great Britain had the industrial mix of the United States. Let $\gamma^B(i)$ and $\mu^B(i)$ be the share of the work force and the union density in industry i in Britain, and let $\gamma^A(i)$ and $\mu^A(i)$ be the same for the United States. In 1974, the aggregate British unionization rate was $\Sigma\gamma^B\mu^B = 49.2$, while union density in the United States was $\Sigma\gamma^A\mu^A = 24.6$. Had Great Britain had the same distribution of workers among industries as the United States, British unionization in 1974 would have been $\Sigma\gamma^A\mu^B = 45.8$. Britain does have a greater share of workers in highly organized industries than the United States, but the different industrial mix accounts for less than 15% of the difference in aggregate union density. In the comparison of union density in the United States and Great Britain, at least, a more disaggregated comparison of the industrial strucure still does not explain much of the difference.

A large share of the cross-national variation of unionization rates can be explained with two variables: the cumulative impact of leftist governments and the size of the labor force. Differences between countries in terms of trade dependence or aggregate measures of the sectoral and occupational distribution of the work force appear much less important, although the data on the proportion of blue-collar workers are too weak to support any strong conclusions regarding the importance of the occupational structure.

These findings contrast sharply with the results of both time series analyses of union growth at the national level and cross-sectional analyses of regional variation within the United States. The size of the labor force changes far too slowly to have a noticeable impact on annual changes of union density. In addition, the size of the local work force has no discernible impact on differences in union density among cities or states within the United States. The relevant labor market for most unions is national in scale, as evidenced by the predominance of unions organized at the national level. Even unions in industries that produce immobile goods, like construction, organize as national unions because of the mobility of workers. Instead, the significant variables in cross-sectional studies within the United States are the sectoral and occupational distribution of the work force, urbanization, and, in some but not all studies, the presence of right-to-work laws (Hirsch and Addison 1986, chap. 3).

The size of the work force does appear, however, to be a prominent explanation of cross-national differences in union density. In 1978 union membership as a proportion of wage and salary earners plus the unemployed in the United States was 24.5%, not very different from the predicted value in Table 3 of 22.6%. The average union density of the other 19 Western democracies was 55.6%. The estimated coefficients in Table 2 indicate that roughly 55%–60% of the gap between the United States and other advanced capitalist countries at the time can be attributed to the exceptional size of the U.S. labor force. The rest of the difference is accounted for by the absence—not unique to the United States—of any governments led by Socialist or Social Democratic parties.

Conclusion

I have introduced a new explanation of cross-national differences in unionization rates: the size of the labor force. Size matters because union power in collective bargaining depends on the proportion of substitutable workers who will respect a strike, while the costs of recruitment depend in part on the absolute number to be recruited. Unions in smaller labor markets can achieve high levels of unionization more cheaply. A comparison of the costs and benefits of organizing new workers yields the conclusion that unions in larger labor markets will accept lower levels of unionization. Statistical analysis of cross-national differences in unionization rates among advanced industrial societies in the late 1970s indicates that the size of the labor force and the cumulative participation of leftist parties in government explain almost three-quarters of the variance.

The reasoning that relates the resources unions devote to recruitment to the size of the labor force is not limited to unions whose primary activity is collective bargaining. In some countries, seeking to influence public policies may be a more consequential activity than negotiating with employers. The political arena may be particularly important in countries like France, where unions lack a strong presence in the work place, as well as in countries like Sweden and Norway, where strong, centralized unions have frequently limited wage demands for the sake of aggregate economic performance.

The political influence of unions depends on many factors, including their ability to strike effectively and their capacity to affect electoral outcomes. In elections—as in strikes—the proportion of union supporters within the relevant population is what matters, not the absolute number of union supporters. While in theory the benefits of mobilizing additional voters might drop sharply once a prounion party gained a majority, in fact there is no advanced industrial democracy where a single party allied with the union movement consistently wins 50% or more of seats in parliament. Whether acting as an interest group or engaged in collective bargaining, the ability to provide benefits for members is likely to increase with the proportion who are union members, while costs depend partly on the number being recruited. The general argument remains applicable.

A negative relationship between the number of potential members and density of membership should also exist for organizations other than trade unions for whom the benefits depend on the proportion who have been recruited while costs depend on number being recruited. For example, the ability of cartels to raise prices depends on the market share controlled by cartel members. If the costs of monitoring compliance increase with the number of producers, cartel members maximize profits by attempting to incorporate a smaller proportion of potential members when the number of potential members is large.

Olson (1965) argued that very small groups may attain high levels of organization because each potential member receives a large share of the benefits of his or her contribution. But Olson's argument cannot explain the negative correlation of group size and the proportion organized among large groups, where individual incentives to contribute in the absence of selective incentives are negligible. In the case of unions and other organizations with structurally similar costs and benefits, the number of potential members may be an important determinant of membership density, not because of the impact of size on individuals' incentive to join but because of the effect of size on organizations' incentive to recruit.

Appendix A: Mathematical Proofs

The union's problem is to choose a nonnegative $z(t)$ to minimize

$$\int_0^\infty e^{-\varrho t}\left(QS[\mu(t),x] + \frac{C[z(t),y]}{m(t)}\right)dt$$

$$\equiv \int_0^\infty J[\mu(t),z(t),t]dt, \qquad \text{(A-1)}$$

where $\mu(t) \equiv m(t)/n$ and $z(t) \equiv dm/dt$. Consider the problem first without constraining $z(t)$ to be nonnegative. The optimal path of $z(t)$ is given by the Euler equation, $\partial J/\partial m = d[\partial J/\partial z]/dt$, or

$$(1/m)C_{zz}(dz/dt) = (1/m^2)C_z z + (\varrho/m)C_z - (1/m^2)C + (Q/n)S_\mu, \qquad \text{(A-2)}$$

where subscripts denote partial differentiation of S and C. In addition the Legendre condition, $\partial^2 J/\partial z^2 = (e^{-\varrho t}/m)C_{zz} \geq 0$, must be satisfied along the optimal path (Tu 1984).

The optimal path of $\mu(t)$ defined by equation A-2 cannot be solved explicitly. However, the stationary value of μ implicit in equation A-2, denoted by μ^*, can be analyzed. Setting $dz/dt = z = 0$ in equation A-2, we have

$$H(\mu^*,\varrho,x,y,n) = (\varrho/m)C_z - (1/m^2)C + (Q/n)S_\mu = 0. \qquad \text{(A-3)}$$

If $C_{zz} > 0$, equation A-2 can be written as a system of first-order differential equations with $dm/dt = z(t)$ and dz/dt as given in equation A-2. Writing the linear approximation in the neighborhood of the stationary point we obtain

$$d\zeta/dt = A\zeta \qquad \text{(A-4)}$$

where ζ is the vector (m,z) and A is a 2×2 matrix with $a_{11} = 0$, $a_{12} = 1$, $a_{21} = (m/C_{zz})(\partial H/\partial m)$, and $a_{22} = (m/C_{zz})(\partial H/\partial z)$. A necessary condition for the existence of a unique optimal path that approaches μ^* from any initial $\mu(0) \neq \mu^*$ is that the stationary solution $(m^*,0)$ be a saddle point or that $|A| = -(m/C_{zz})(\partial H/\partial m) < 0$. Thus we have the additional condition that

$$\frac{\partial H}{\partial m} = -(\varrho/m^2)C_z + (2/m^3)C + (Q/n^2)S_{\mu\mu} > 0 \qquad \text{(A-5)}$$

at the stationary point.

If equation A-5 holds and $\mu(0) < \mu^*$, $z(t) \geq 0$ everywhere along the optimal path and the constraint that z be nonnegative is never binding. Thus the solution to the constrained problem is identical to the solution to the unconstrained problem. If $\mu(0) > \mu^*$ the optimal solution to the unconstrained problem implies $z < 0$ as $\mu(t)$ approaches μ^*. In this case, the solution to the constrained problem is simply $z(t) = 0$ and $\mu(t) = \mu(0)$ as long as the parameters ϱ, x, y, and n remain constant. (This failure of $\mu(t)$ to converge to μ^* when $\mu(0) > \mu^*$ is a consequence of the simplifying assumption that $dm/dt = z(t)$. In the more general formulation with $dm/dt = z(t) - \delta m(t)$, where δ represents the net loss of membership that occurs when unions do not recruit new members, the optimal path of $\mu(t)$ may approach μ^* from any initial value when $\delta > 0$.)

Propositions 1–3 in the text are now readily proven. Differentiating H with respect to ϱ—treating μ^* as an implicit function of ϱ—yields

$$\frac{d\mu^*}{d\varrho} = -(\partial H/\partial m)^{-1}(1/mn)C_z < 0 \tag{A-6}$$

since $C_z > 0$.[24] Differentiating H with respect to x yields

$$\frac{d\mu^*}{dx} = -(\partial H/\partial m)^{-1}(Q/n^2)S_{\mu x} > 0 \tag{A-7}$$

if and only if $S_{\mu x} < 0$. Differentiating H with respect to y yields

$$\frac{d\mu^*}{dy} = -(\partial H/\partial m)^{-1}[(\varrho/mn)C_{zy} - (1/m^2n)C_y], \tag{A-8}$$

which may be positive or negative. If, however, $C(z,y) = yc(z)$ with $y > 0$, then $(\varrho/mn)C_{zy} - (1/m^2n)C_y = (\varrho/mn)c'(z) - (1/m^2n)c(z) = -(Q/yn^2)S_\mu$ by equation A-3. In this case

$$\frac{d\mu^*}{dy} = (\partial H/\partial m)^{-1}(Q/yn^2)S_\mu < 0 \tag{A-9}$$

since $S_\mu < 0$.

To prove proposition 4, start with the impact of the size of the labor force on the stationary size of the union:

$$\frac{dm^*}{dn} = (\partial H/\partial m)^{-1}(Q/n^2)(\mu S_{\mu\mu} + S_\mu), \tag{A-10}$$

which will be positive if $-(\mu S_{\mu\mu}/S_\mu) > 1$ and negative if $-(\mu S_{\mu\mu}/S_\mu) < 1$, where $-(\mu S_{\mu\mu}/S_\mu)$ is the elasticity of S_μ with respect to μ. However, the effect of n on the stationary union density does not depend on the elasticity of S_μ:

$$\begin{aligned}\frac{d\mu^*}{dn} &= (1/n^2)[(dm^*/dn)n - m] \\ &= (1/n^2)(\partial H/\partial m)^{-1}[(Q/n)S_\mu + (\varrho/m)C_z - (2/m^2)C] \\ &= -(1/nm)^2(\partial H/\partial m)^{-1}C \\ &< 0\end{aligned} \tag{A-11}$$

since $(Q/n)S_\mu = -(\varrho/m)C_z + (1/m^2)C$ from equation (A-3).

If $C_{zz} = 0$, then $C(z,y)$ must have the form $C(z,y) = C_0(y) + C_1(y)z$. Straightforward calculation reveals that all of the above results continue to hold provided equation A-5 is satisfied.

Appendix B: Data Sources

Union membership. U.S. Department of Labor 1980 for all countries other than Iceland and Switzerland. Coldrick and Jones 1979 (p. 876) has membership figures for Iceland. See Troy and Sheflin 1985 (pp. 7–17) for Switzerland.

Potential membership (wage and salary earners plus the unemployed). Organization for Economic Cooperation and Development (OECD) 1984 for all countries but Israel, for which see Israel, Central Bureau of Statistics 1980 (p. 300).

Left government. Wilensky's index of leftist party participation in government is defined in Wilensky 1981. The composition of government coalitions from 1919 to 1980 is from Wallerstein 1988a. Mackie and Rose 1982 is the source for shares of seats in parliament.

Merchandise exports as a proportion of GDP in 1980. Export figures are from United Nations, Food and Agriculture Administration 1985. United Nations 1985 has GDP figures.

Employment in mining, manufacturing, utilities, and construction as a share of civilian employment in 1980. OECD 1984 (p. 34) for all but Israel, for which see Israel, Central Bureau of Statistics 1980 (pp. 316–317).

Production workers, transport and equipment operators, and laborers as a proportion of economically active population. International Labour Office (ILO) 1979–83. Census data, as opposed to labor force survey data, was used whenever possible. Belgium and Iceland were excluded for lack of data. Italy was excluded because the figure reported by the ILO is implausibly low.

Union centralization. Dummy variable set equal to one for countries with highly centralized confederations according to Headey's (1970) classification. The Icelandic labor confederation was judged to be decentralized (U.S. Bureau of Labor Statistics 1956).

Notes

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1. The direction of causality is unclear. This question is discussed below.

2. Collecting data on union density is fraught with difficulties (Bain and Price 1980, chap. 1). Membership figures are provided by the unions with the inevitable result that their reliability varies from country to country. Union members are defined as the people listed on the unions' membership rolls. This is generally close to but larger than the number of people who pay union dues: apprentices and unemployed and retired workers are often exempted from the payment of dues, and all unions allow members to be in arrears for a period of time before being struck from the list. In France, however, the proportion of workers on the membership rolls who pay dues is probably around 10% (Adam 1983). In Israel, where individuals who are not economically active may join the General Federation of Labor (Histadrut) to gain access to the Histadrut's health care system, union membership exceeds the number of wage and salary earners by 50%. Unions may have an incentive to inflate their membership figures to impress employers or the government or to increase their representation in the national union confederation. On the other hand, unions also have an incentive to deflate their membership figures in order to reduce the dues owed the confederation. Goldfield (1987, 9) suggests that some of the reported decline in union density in the United States in the 1970s may be due to an increase in underreporting following large increases in the per capita taxes assessed by the AFL-CIO. Membership figures often include retired workers who maintain their membership and thus overstate the proportion of the work force that is unionized. At the same time, membership figures are often understated in that members of unions not affiliated with the major union confederations are excluded. In sum, in spite of the measurement error, there appears to be no clear bias in union membership figures. An additional difficulty in comparing union density figures stems from differences in the definition of the denominator. For this reason I collected figures for union membership, not union density, and divided by the number of wage and salary earners plus the unemployed for all countries except Israel.

3. This was not true prior to the Second World War. In Germany the unionization rate quadrupled from 1918 to 1920, then fell by one-half from 1922 to 1925. British union density tripled between 1910 and 1920 and fell by 50% in the subsequent decade 1920–30 (Bain and Price 1980). Of the eight countries studied by Bain and Price—Australia, Denmark, Canada, Germany, Sweden, the United Kingdom, and the United States—the United States and Canada had the lowest and second lowest union densities throughout the interwar period. Union density in Sweden, however, did not surpass German or British levels until the mid-1920s.

4. This discussion of the literature on union growth is drawn from a survey by Przeworski (1984).

5. This is no longer the case in the field of industrial relations. Block (1980), Freeman and Medoff (1984), and Goldfield (1987) all argue that a decrease in organizing effort by unions is part of the explanation of the postwar decline in union density in the United States.

6. The assumption that workers receive the same income when on strike as when laid off is adopted purely for simplicity of exposition. Allowing the two to differ does not change my analytic conclusions significantly. Workers' reservation wage r is assumed to be strictly positive.

7. Similarly, the assumption that workers care

only about their expected income rather than a variety of pecuniary and nonpecuniary benefits such as seniority rights, grievance procedures, and working conditions is an inconsequential simplification. In the present context, all union demands that raise labor costs are equivalent.

8. Nash (1950) proved that the point on the feasibility set that maximizes the weighted geometric average of the bargaining gains is the unique solution that satisfies the axioms of Pareto optimality, the independence of affine transformations of the payoffs, and the independence of irrelevant alternatives (or, more appropriately, the irrelevance of unchosen alternatives). Other cooperative bargaining solutions drop one or more of Nash's axioms (except for Pareto optimality) and add other axioms until a different unique solution is defined. A nonaxiomatic approach to bargaining is developed by Rubinstein (1982), who reformulates the bargaining problem as a sequential game where offers and counteroffers are made until an agreement is reached. See Kalai 1985 for a recent review of cooperative bargaining theory and Sutton 1986 for an introduction to noncooperative bargaining theory. Note that the solutions differ in the case in which $L > M$ or in which the demand for labor in the plant exceeds the union local's membership (Wallerstein 1987).

9. See Wallerstein 1987 for a more rigorous derivation of the Nash and proportional solutions for this model with $\Theta(L) = \min(1, L/M)$.

10. This is not true if unions maximize the expected utility of risk-adverse workers (McDonald and Solow 1981). However Clark (1984) finds evidence that labor contracts in the United States do satisfy this condition.

11. In the noncooperative model, α approaches $\xi/(\varrho + \xi)$ as the time between making an offer and receiving a counteroffer goes to zero where ξ is the discount rate of the firm and ϱ is the discount rate of the union (Sutton 1986).

12. Freeman and Medoff (1981, 567) estimate that an increase of 10 percentage points in union density at the industry level raises union wages by approximately 1.5% in U.S. manufacturing. They found no impact of union density at the industry level on nonunion wages.

13. This calculation is based on the rough figure that union membership would have had to increase by 2.4 million—185,000 per year—between 1964 and 1977 to maintain union density at its 1964 level (Goldfield 1987, 10). Voos (1984b, 56) presents organizing expenditures from 1953 to 1974 by 20 unions who collectively represent about half of the union members in the United States. I doubled Voos's figures and converted from 1967 to 1980 dollars according to the CPI to arrive at a ballpark estimate that total organizing expenditures were about $200 million in 1980 dollars per year.

14. In previous work I allowed costs to depend also on the level of unionization in accordance with the saturation effect found in time series models of union growth. Since the saturation effect added nothing essential to the mathematical analysis, I removed it to avoid unnecessary complexity. See Wallerstein 1988b for details.

15. The infinite time horizon is not critical. Letting the time horizon be finite and other modifications are discussed below.

16. It is also true that the costs of organizing in industries with high turnover are greater.

17. If x is the price of the firm's output, $S(\mu,x) = xs(\mu)$, where $s(\mu)$ is the output that could be produced during a strike. Then $\partial^2 S/\partial\mu\partial x = \partial s/\partial\mu < 0$.

18. Ellwood and Fine (1987) find large decreases in union organizing at the state level after the passage of right-to-work laws.

19. Using the raw size of the labor force would indicate that the addition of one million new workers would have the same impact whether it increased the labor force by 100% or only 5%.

20. The index, developed by Wilensky (1981), gives each country from zero to three points for each year of leftist party participation in government depending on the Left's share of the parliamentary seats held by members of the governing coalition, the party of the prime minister, and whether the government has a majority in parliament. (There were no governments with leftist party participation in the presidential systems of the United States and the French Fifth Republic during the years of the index. Finland was treated as a purely parliamentary system.) Leftist parties include all Communist, Socialist, Social Democratic and Labor Parties with the exception of the Socialist and Social Democratic Parties of Italy. Table 3 presents the country scores. An index of leftist party participation at the national level is a standard measure of the influence of leftist parties on public policy. Nevertheless, it is admittedly a rough measure that misses more subtle distinctions, such as the impact of leftist governments at the state or provincial level (possibly important in the Canadian case) or the difference between governments formed by center parties as opposed to conservative parties.

21. After controlling for leftist governments and the size of the work force, union centralization is not significantly associated with union density. In contrast, the centralization of the union movement is frequently cited as contributing to the electoral success of social democratic parties (Korpi 1983; Stephens 1980).

22. Goldfield (1987, chap. 7), in contrast, does not find a consistent relationship between occupational or industrial variables and the likelihood of union victory in National Labor Relations Board elections. Goldfield's result is exactly what one would expect if unions allocated their organizing efforts efficiently: all subgroups are roughly equally likely to support unionization at the margin.

23. The sectors of the economy are metal and

machinery; clothing; food, drink, and tobacco; paper, printing, and publishing; leather and leather products; textiles; lumber and lumber products; chemicals, rubber, clay, glass, and stone; the residual manufacturing sector; transportation, utilities, and communication; construction; mining, quarrying, and oil; central government (excluding health care in Britain); state and local government, including education; and the residual (service sector).

24. This equation assumes that $Q = \alpha/M$ is independent of ϱ, as in cooperative bargaining theory. In noncooperative bargaining theory, $d\alpha/d\varrho < 0$, in which case $\partial H/\partial\varrho = (1/m)C_z + (1/Mn)S_\mu(d\alpha/d\varrho)$. The term $\partial H/\partial\varrho$ is still positive, since S_μ and $d\alpha/d\varrho$ are both negative. Therefore, the sign of $d\mu^*/d\varrho$ is not altered.

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NUCLEAR DETERRENCE AND THE STRATEGY OF LIMITED RETALIATION

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Recent formal work in nuclear deterrence theory has focused on brinkmanship crises in which states exert coercive pressure by manipulating the risk of an unlimited nuclear exchange. This essay extends the formal analysis of deterrence theory to the strategy of limited retaliation in which states exert coercive pressure by inflicting limited amounts of damage in order to make the threat of future punishment more credible. This strategy is modeled as a game of sequential bargaining with incomplete information. The equilibria suggest that states prefer relatively smaller, less-destructive limited options; that counterforce options are desirable even if they cannot limit the total amount of damage an adversary can inflict; that smaller, less-destructive limited nuclear options may make a nuclear exchange more likely; and that uncertainty and incomplete information may significantly enhance deterrence.

If both superpowers have secure second-strike capabilities, each can absorb a first strike and then destroy the other with a retaliatory second strike. In this technological condition of mutually assured destruction, there are at least two approaches to deterrence. The first, brinkmanship, is based on manipulating the risk that a crisis will go out of control and will end in an unlimited nuclear exchange. In a brinkmanship crisis, each state tries to coerce its adversary into backing down by raising the risk of an unlimited exchange to a level that its adversary finds intolerably high (Jervis 1979, 1984; Kahn 1965; Powell 1985; Schelling 1960, 1962a, 1962b, 1966). The second approach to deterrence is based on a strategy of limited retaliation. In this approach each state tries to coerce its adversary into backing down by inflicting a limited amount of punishment in order to make the threat of future punishment sufficiently credible that the adversary no longer finds it worthwhile to continue the confrontation (Davis 1976; Halperin 1963; Kaplan 1962; Knorr 1962; Osgood 1979; Powell 1985; Schelling 1962a, 1965, 1966).

Recent formal work (Nalebuff 1986; Powell 1987, 1988) has used games of incomplete information and the notion of sequential or perfect equilibria to examine the escalatory dynamics of nuclear brinkmanship. I extend the formal analysis of strategic nuclear deterrence to the study of the strategy of limited retaliation by modeling this strategy as a game of sequential bargaining with incomplete information. The game is sufficiently simple that its sequential equilibria may be characterized explicitly and then used to study the dynamics of escalation in this strategy. The analysis suggests four conclusions. First, each state prefers to have a large number of limited options, that is, options that if exercised, cause relatively little damage. Second, each state also

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wants to have counterforce options. This conclusion is surprising because each state is assumed to be able to destroy the other state throughout the confrontation. Counterforce options are, by assumption, incapable of helping a state limit the damage that an adversary is capable of inflicting if it decides to do so. Nevertheless, counterforce options are still desirable. The third conclusion is related to the effects of smaller, less-destructive limited options on crisis stability. It is argued both that smaller, less-destructive limited options make nuclear war less "unthinkable" and therefore more likely (Panofsky 1973; Scoville 1974) and that they make a threat to retaliate more credible and thus enhance deterrence and make war less likely (Greenwood and Nacht 1974; Kahn 1962; Schlesinger 1974). In the model a large reduction in the destructiveness of a state's limited options may reduce the probability of a nuclear exchange, but a small reduction may make a nuclear exchange—albeit a less destructive one—more likely. Finally, the analysis shows that uncertainty about an adversary's resolve may play a crucial role in enhancing deterrence by preventing challenges to the status quo that would be certain to occur otherwise.

The analysis begins with a discussion of the strategy of limited retaliation and its relation to brinkmanship. Limited retaliation is then examined in the context of a game of complete information. Thereafter, I consider the effects of introducing incomplete information into the model.

The Strategy of Limited Retaliation

The strategy of limited retaliation attempted to solve the credibility problem inherent in the doctrine of massive retaliation once both superpowers acquired secure second-strike strategic forces. In the doctrine of massive retaliation, the United States tried to protect the entire spectrum of U.S. interests ranging from the most peripheral to the most vital by threatening to launch a massive nuclear attack in retaliation to a challenge to any of these interests.[1] This doctrine was immediately criticized as being an ineffectual means of protecting peripheral U.S. interests (Kaufmann 1956). The threat to retaliate massively to a challenge to these interests was simply not credible. Nevertheless, as long as the United States remained relatively invulnerable to a Soviet nuclear attack, the doctrine was not criticized as a means of protecting vital interests like Western Europe.

Once both superpowers acquired second-strike forces, the doctrine of massive retaliation appeared ineffectual even if used to protect only vital interests. Carrying out the threat to launch a massive nuclear attack would mean one's own destruction. The cost of implementing the threat was greater than the cost of not doing so; thus the threat seemed inherently incredible. The doctrine of massive retaliation foundered on the problem of credibility.

Nuclear deterrence theory offered two general solutions to the problem of credibility. The first is based on brinkmanship and Thomas Schelling's (1960, 1962b, 1966) threats-that-leave-something-to-chance. If a state could not credibly threaten to impose the sanction of a massive nuclear retaliatory attack deliberately, perhaps it could make threats-that-leave-something-to-chance; that is, a state could threaten to take steps that created not a certainty but only a risk or chance that the state would launch a massive attack. In other words, during a crisis a state could exercise a limited option that created some risk that the crisis would go out of control and end in an unlimited nuclear exchange. Because the exercise of these limited options only created a risk of a general nuclear exchange and did not make it certain, a state might rationally decide to use a limited option if the risk it generated was sufficiently low and the cost of submitting to its adversary was sufficiently high. Crises then would become a competition in taking risks in which each state bid up the risk until one state or the other found the risk intolerable and submitted or until the risk of disaster became a reality and there was a general nuclear exchange. By providing a means of exerting coercive power through credible threats to manipulate the risk of a massive attack, Schelling's threats-that-left-something-to-chance offered at least a conceptual solution to the credibility problem.

Although brinkmanship and its associated threats-that-leave-something-to-chance solved the credibility problem in principle, the solution was not entirely satisfactory. Both the doctrine of massive retaliation and brinkmanship rely on the same sanction, a massive nuclear attack. With no state willing to impose the sanction deliberately, brinkmanship had to appeal to an "autonomous risk" (Snyder and Diesing 1977, 210) that this sanction would be imposed (Freedman 1986; Nalebuff 1986; Powell 1985, 1988; Rhodes 1988; Schelling 1960, 1962b, 1966); that is, there had to be some chance that a state would launch a massive nuclear attack although it was not rational to do so. At root brinkmanship depended on manipulating the risk that a massive nuclear attack would be launched accidentally or irrationally. Depending on this to deter an adversary from challenging vital interests seemed somewhat unsatisfactory. "To rely on leaving things to chance, however realistic in terms of the actual fears and perceptions of political leaders and the difficulty of controlling the process of escalation once it was under way, seemed like the abandonment of strategy" (Freedman 1986, 773).

The strategy of limited retaliation pursued a second approach to the credibility problem inherent in the doctrine of massive retaliation (Davis 1976; Kaplan 1962; Knorr 1962; Powell 1985; Schelling 1962a, 1962b, 1965, 1966). By appealing to a sanction that no state could credibly threaten to impose deliberately, brinkmanship was forced to rely on threats-that-left-something-to-chance and on the autonomous risk implicit in them. The strategy of limited retaliation was to be based on a set of more limited sanctions that a state could credibly threaten to impose deliberately. During a crisis, a state would attempt to deter an adversary from taking some action by threatening to inflict a limited amount of punishment on this adversary. Should this threat initially prove insufficiently credible to deter an adversary, then a state would actually inflict some punishment in order to make the threat of future punishment more credible.

These limited options had to satisfy two criteria: a state had to be able to impose costs high enough to induce the adversary to back down rather than endure the limited punishment, but the options had to be sufficiently limited, so that even if they were exercised, the adversary would still be left with something else to lose. This was the key to the credibility problem. An adversary might be deterred from retaliating in order not to lose what was left. A state might, for example, threaten to destroy one of its adversary's cities in order to coerce it into backing down during a severe confrontation in which vital national interests were at stake. If during the course of this confrontation this threat were carried out, then—despite the horrendous loss of a city—the adversary would still have much left to lose. Moreover, the fact that it had already lost one city might make the threat that it was about to lose another very credible. This, in turn, might convince it not to retaliate and to back down.

Of course, both superpowers could employ the strategy of limited retaliation; that is, each state could attempt to coerce its adversary into backing down by inflicting limited amounts of punishment in order to convince its adversary that the cost of continuing would be greater than the cost of backing down. In this way a crisis would become a contest of resolve in which each state attempted to demonstrate that it was more willing to inflict and endure punishment in order to have its way than was its adversary. In the end one of the states might succeed in making the threat of future damage sufficiently credible to induce its adversary to back down. Or each state might continue to believe that if it holds on just a little longer and inflicted just a little more damage, its adversary would quit. In this case, each state would destroy the other: "A war of endurance might bleed both sides to death" (Schelling 1962b, 429).

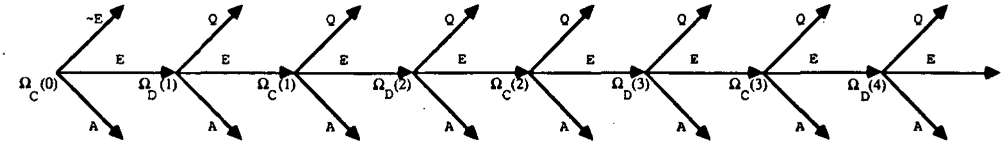
The final result of brinkmanship and the strategy of limited retaliation might be the same, the utter devastation of both states. But the essence of the dynamic is very different. Brinkmanship depends on the autonomous risk that the crisis will go out of control. During a crisis the states exert coercive pressure on each other by manipulating this risk. Crises are a competition in taking autonomous risks. In the strategy of limited retaliation the states remain in complete control and, while in complete control, begin to destroy each other through a series of limited attacks. Through these attacks the states exert coercive pressure on each other by attempting to make the threat of future destruction sufficiently credible to induce the adversary to back down. Each of these limited attacks is launched rationally in the sense that each state believes that the chance of the adversary's soon backing down is high enough to make it worth holding on just a little longer. Crises in the strategy of limited retaliation are a competition in inflicting punishment.

Before turning to a model of the strategy of limited retaliation, I shall briefly relate the strategy of limited retaliation to the counterforce doctrines that have played such a prominent role in the evolution of U.S. nuclear strategy. These doctrines are based on counterforce capabilities and escalation dominance and have in one form or another been part of McNamara's "no-cities" doctrine of 1962, Schlesinger's doctrine of limited nuclear options in 1974, and the Carter administration's countervailing strategy, which was formalized in presidential directive 59 in 1980 (Ball 1982; Davis 1976; Freedman 1986; Sagan 1989; Slocomb 1981).

By these doctrines a state may use its counterforce capabilities partly in order to limit damage to itself. This, however, is not the primary purpose of these capabilities, for both superpowers are assumed to be able to destroy each other; that is, a state cannot physically protect its cities should its adversary decide to attack them. Rather, counterforce capabilities are to be used to keep the onus of escalation on an adversary. If, that is, a state has escalation dominance, then regardless of the level of the military confrontation, a state can use its counterforce capabilities to ensure that its adversary is very unlikely to prevail at the current level of military conflict. Consequently, the adversary is continually forced to decide whether it will back down or escalate to a still higher level of violence. By forcing its adversary to bear the burden of escalation at every level, a state hopes that its adversary will eventually find this burden too great and will back down.[2]

But in deciding whether or not to back down, a state still has the ability to destroy its adversary's cities. If it can do this in a limited way, this state still has the option of turning to a strategy of limited retaliation at any stage in the counterforce struggle. As Schelling (1965; 1966, 190–204) recognized, counterforce doctrines are in the end likely to confront a state with the same dilemma as does the strategy of limited retaliation. Does a state

Figure 1. Limited Retaliation with Complete Information



surrender a vital national interest or engage in a horrible campaign of destruction in an attempt to coerce the other state into surrendering? Because the fundamental dilemma is the same, a better understanding of this dilemma and the dynamics of the strategy of limited retaliation is also relevant to these counterforce doctrines.

The Complete-Information Model

Before describing the model, an important qualification is in order. To the extent that one envisions the use of a large number and wide range of limited nuclear options, the strategy of limited retaliation makes very strong assumptions about each state's ability to command and control its nuclear forces during a conflict. Recent work on the command and control system (e.g., Blair 1985; Bracken 1984; Carter, Steinbruner, and Zracket 1987) raises very grave doubts about such an assumption. As a model of the strategy of limited retaliation, this strong assumption will also be implicit in the model. As such, the model is at most a first step toward an analysis that integrates aspects of both limited retaliation and brinkmanship.

The strategy of limited retaliation is modeled as a game in which each state tries to coerce its adversary into submission by inflicting limited amounts of punishment on it. In the game, which is illustrated in Figure 1, there are two players: a challenger, *C*, and a defender, *D*. The challenger and defender start with some initial level of resources, r_C and r_D, respectively. In the gruesome story underlying the strategy of limited retaliation and the model, r_C may be thought of as the number of cities C has and r_D as the number of *D*'s cities.

Play begins with *C*'s having to choose one of three alternatives. It can refrain from challenging the status quo by playing $\sim E$ in which case the game ends immediately. Or it can launch an unlimited nuclear attack against *D* by choosing *A*. This too ends the game: *C*'s unlimited attack destroys *D* and *D* is assumed to destroy *C* in retaliation. Finally, *C* may exploit the situation by challenging the status quo with *E*. This shifts the onus of escalation onto *D*.

C's challenge forces *D* to decide which of three courses to follow: *D* can submit to *C*'s challenge by quitting the crisis with *Q*, launch an unlimited attack against *C* by following *A*, which ends the game with both states' utter destruction, or escalate *E* by launching a limited attack against *C*, that is, destroy part of what *C* values. In that case let p_D be the amount of punishment inflicted on *C* by *D*'s limited attack. Then, *D*'s limited attack leaves $r_C - p_D$ of *C* intact.

D's limited attack shifts the onus of escalation back to *C*. *C* must now decide among the three options that just confronted *D*: submit with *Q* and end the game, end the game by launching an unlimited attack against *D*, launch a limited attack against *D* with *E*. The limited attack destroys part of what *D* values and leaves *D* with $r_D - p_C$, where p_C is the amount of punishment inflicted by C's limited attack.

C's limited attack shifts the onus of escalation back to D. As before, D must decide whether it will quit the crisis by submitting to C, launch an unlimited attack, or carry out a limited attack. A limited attack again destroys p_D of C. After D's second limited attack, C is left with $r_C - 2p_D$.

The game continues in this way with the onus of escalation shifting back and forth. C's mth limited attack leaves $r_D - mp_C$ of D intact, and D's mth attack leaves $r_C - mp_D$ of C intact. Once it starts, escalation can end in only one of three ways: one of the states could end the crisis by submitting to its adversary; one of the states could end the game at some point by launching an unlimited nuclear attack; or the states could bleed each other to death. That is, each state might continue to escalate by launching limited attacks against its adversary until one of them is completely destroyed, albeit incrementally. Symbolically, such attacks might continue until $r_C - M_D p_D = 0$ or $r_C - M_C p_C = 0$, where M_D is the number of D's limited attacks after which C will have nothing left to lose and M_C is the number of C's limited attacks after which D will have nothing left to lose. (To simplify matters, r_C and r_D are assumed to be evenly divisible by p_D and p_C, respectively.) Once one of the states, say C, is completely destroyed—that is, once $m = M_D$ so that $r_C - mp_D = 0$—it has nothing left to lose and is assumed to destroy D in retaliation. Thus, the game can continue only as long as both states have something left to lose. As soon as one of the states has nothing left to lose, it destroys its adversary and the game ends. In terms of the game tree this means that the last decision node in the tree is characterized by a choice between quitting or completely destroying an adversary. D's last decision node in Figure 1, for example, is the last decision node precisely because at this point all that remains intact of C is p_D and D must choose between quitting and attacking. If D quits, the game ends with D's submission. If, however, D attacks, C is left with nothing more to lose and is assumed to retaliate against D with an unlimited attack. The game ends in the complete destruction of both C and D.

It will be convenient to use the following notation for the states' strategies and information sets, which, in the complete information game, are individual decision nodes. Let $\Omega_D(m)$ be the information set at which the defender must carry out the mth limited attack on C if it is to escalate. Similarly, $\Omega_C(m)$ for $m \geq 1$ is the information set at which C must carry out the mth limited attack on D if it is to escalate. At $\Omega_C(0)$ C is deciding whether or not to dispute the status quo. C has to choose one of three alternatives at $\Omega_C(m)$: it can quit, escalate by launching a limited attack, or launch an unlimited attack. Consequently, C's behavioral strategy at $\Omega_C(m)$ can be described by the probability that C will escalate—$e_C(m)$—and the probability that C will launch an unlimited attack—$a_C(m)$. The probability of C's quitting the crisis is given by $1 - e_C(m) - a_C(m)$. D's behavioral strategies are labeled similarly. At $\Omega_D(m)$, D escalates with a limited attack with probability $e_D(m)$, launches an unlimited attack with probability $a_D(m)$, and quits with probability $1 - e_D(m) - a_D(m)$.

To complete the description of the game, the payoffs must be specified. This specification is necessarily somewhat arbitrary. There are no clear historical parallels to the situation underlying the strategy of limited retaliation that could be used to inform the specification of the payoffs. Nor are there any widely accepted analogies in the existing work on limited retaliation that could help to specify the payoffs. Fortunately, the equilibrium outcomes of, at least, the complete information game turn out to be quite insensitive to the precise specification of the payoffs.

The game can end in one of three ways. The game could end with the continuation of the status quo if C decides against making a challenge. Let the payoff to this be (r_C, r_D), where the first element in the ordered pair is the challenger's payoff. The two states could also completely destroy each other either because one of them launches an unlimited attack or because they bleed each other to death. Normalize the payoff to this outcome to be $(0,0)$. Or one of the states could submit. Suppose that the challenger prevails because the defender submits. In this outcome the challenger's payoff should reflect two opposing influences: the gain due to prevailing and the loss due to the limited punishment that D may have wrought on C. The precise payoff to the challenger if it prevails after suffering m limited attacks will be taken to be $(r_C - mp_D) + (1 - mp_D/r_C)w_C$. This expression is motivated by two considerations. First, note that if C challenges the status quo and D then submits without attacking, $m = 0$ and the payoff to C is $r_C + w_C$. The value of the status quo for C being r_C, w_C is the net gain that C can derive from changing the status quo while at the same time being able to deter D from inflicting any punishment on C. In this sense w_C is the marginal gain of prevailing without resistance. More generally, if C prevails after having suffered m limited attacks, the first term is simply how much of C is left intact when D submits. The second term is the incremental gain due to prevailing. Note that the size of the gain depends on m. The greater m, that is, the more damage C has suffered, the smaller the incremental gain of prevailing. The idea here is that the more destruction C has suffered, the less able it is to exploit the fruits of its "victory." In the extreme, if C is bled to death—that is, if $r_C - mp_D = 0$—the incremental gain of prevailing will be zero: $[1 - m(p_D/r_C)]w_C = 0$. Prevailing is worthless to one who has been bled to death.

Similarly, the payoff to D of submitting after C has attacked m times is taken to be $(r_D - mp_C) - (1 - mp_C/r_D)s_D$, where s_D is the incremental loss that D will suffer if it does not resist C's initial challenge; that is, if D submits as soon as C challenges the status quo, its payoff will be $r_D - s_D$ and its incremental loss will be s_D. Again, the first term reflects how much of D remains when the confrontation ends. The second term is the incremental loss of submitting.

Finally, suppose that the game ends because C submits to D. The payoff to D of prevailing after having endured m limited attacks is $(r_D - mp_C) + (1 - mp_C/r_D)w_D$, where w_D is the incremental gain to D if it can prevail by coercing C into submitting after C has challenged the status quo but before it has carried out a limited attack against D. As with C, the incremental gain that D derives from prevailing declines as D suffers more damage. The payoff to C of submitting after having been attacked m times is $(r_C - mp_D) - (1 - mp_D/r_C)s_C$, where s_C reflects the incremental cost of submitting.

I will also assume that the payoff to prevailing after having been attacked m times is greater than the payoff to submitting after having been attacked m times. This implies that $w_C + s_C > 0$ and $w_D + s_D > 0$. And as long as a state has something left to lose, submitting will be assumed to be better than being completely destroyed. This leaves $r_C - s_C > 0$ and $r_D - s_D > 0$. Finally, it will also be convenient to assume that the difference between winning and losing is less than the absolute payoff to losing; that is, $r_C - s_C > w_D + s_D$ and $r_D - s_D > w_D + s_D$.

Thinking of the game as a rather unusual auction may help to clarify its basic structure. C starts with r_C dollars and D with r_D dollars. Initially the auctioneer asks C if it wants to participate in the auction; D has no choice in the matter. If C

does not, there is no auction and C and D are left with r_C and r_D. If C wants to participate, the auction begins. The object of the auction is to coerce one's adversary into submitting, that is, passing a turn to bid. D has the first bid. If it passes, the auction ends with C's receiving w_C and therefore ending up with $r_C + w_C$. D has to pay s_D and ends with $r_D - s_D$. If D decides to bid, the auctioneer subtracts p_D from C. C is now left with $r_C - p_D$ and must decide whether or not to bid. If C passes, the game ends. D receives w_D and leaves the auction with $r_D + w_D$. C pays an additional $[1 - (p_D/r_C)]s_C$ for passing after D has bid once and is left with a total of $r_C - p_D - [1 - (p_D/r_C)]s_C$. If, instead, C bids, p_C is subtracted from D, leaving it with $r_D - p_C$. If D now passes, the game ends and an additional $[1 - (p_C/r_D)]s_D$ is subtracted from D, which leaves it with a total of $r_D - p_C - [1 - (p_C/r_D)]s_D$. C receives $[1 - (p_D/r_C)]w_C$ for prevailing and ends the game with $r_C - p_D + [1 - (p_D/r_C)]w_C$. If D had bid a second time, p_D would be subtracted from C a second time, leaving it with $r_C - 2p_D$. It is now C's turn to bid. If C passes, D receives $[1 - (p_C/r_D)]w_D$, leaving it with $r_D - p_C + [1 - (p_C/r_D)]w_D$, and C loses $[1 - 2(p_D/r_C)]s_C$, which gives it a total of $r_C - 2p_D - [1 - 2(p_D/r_C)]s_C$. If C bids for a second time, an additional p_C is subtracted from D leaving it with $r_D - 2p_C$. The auction continues in this way until one of the bidders passes. If no bidder passes, one of them eventually runs out of money. At this point the auctioneer ends the auction by subtracting from the other bidder whatever money it still has and both are left with nothing.

This auction differs from the dollar auction that has been used to study escalation (O'Neill 1986; Shubik 1971) in a subtle yet crucial way. In the dollar auction, two players bid for a dollar. The bidding alternates back and forth until one of the players passes. At this point the dollar is awarded to the player who did not pass. The important feature of this auction, however, is that regardless of which player wins the dollar, both players pay the auctioneer their bids. If, for example, one player has bid 75 cents and its opponent, having previously bid 50 cents, passes, the first player receives the dollar and pays 75 cents for a net gain of 25 cents, while the second player pays the value of its bid and loses 50 cents.

Shubik (1971) and O'Neill (1986) offer the dollar auction as a model of escalation and note that in practice the bids often exceed a dollar; that is, individual players often end up paying more than the value of winning. Schelling explicitly appeals to a variant of the dollar auction as an analogy for the strategy of limited retaliation: "In early days, San Franciscans, it is said, conducted 'duels' by throwing gold coins one by one into the Bay until one or the other called it quits" (Schelling 1962a, 243–44).

The dollar auction, however, is a problematic analogy for the strategy of limited retaliation. In limited retaliation a state "bids" by attacking its adversary and destroying part of it. A state does not pay the price of its "bid," its adversary does. This is in keeping with my auction but does not accord well with the dollar auction. In the dollar auction the bidder, not its adversary, pays the price of the bid. Accordingly, my auction would seem to provide a better analogy for the strategy of limited retaliation than does the dollar auction.

The Complete Information Equilibria

The sequential equilibria of the complete information game suggest that each state would like to have a large number of limited options, each of which would, if exercised, inflict relatively little punishment on an adversary. Moreover, each state would also like to have counterforce options. Surprisingly, counterforce options are still desirable even though a state cannot use them to limit the total amount of damage that an adversary can inflict. Indeed, throughout the confrontation, each state is always able to destroy its adversary by launching an unlimited nuclear attack.

Focusing on sequential equilibria solves the credibility problem inherent in the doctrine of massive retaliation. This doctrine foundered because it was based on an inherently incredible threat, that is, on a threat that would not be rational to carry out. Sequential equilibria address this problem by formally precluding strategies that would be irrational to implement, given the player's beliefs when the decision whether to carry out the threat must be made. In this sense, then, sequential equilibrium strategies and the threats implicit in them are credible.[3]

The number of a state's limited attacks after which its adversary will have nothing left to lose determines which state owns the last decision node in the game. The ownership of this node, in turn, plays a crucial role in the analysis of the game's equilibria. Recall that M_C is the number of C's limited attacks after which D will have nothing more to lose. Having nothing left to lose means that $r_D - M_C p_C = 0$. This gives $M_C = r_D/p_C$, where, as noted earlier, r_D is assumed to be evenly divisible by p_C. Similarly, M_D is the number of D's limited attacks after which C will have nothing left to lose, and $M_D = r_C/p_D$.

To see how M_C and M_D determine which state owns the last decision node in the game, suppose $M_C \geq M_D$. Then after each state has attacked $M_D - 1$ times C will have $r_C - (M_D - 1)p_D > 0$ and D will have $r_D - (M_D - 1)p_C > 0$. Substituting r_C/p_D for M_D, C has p_D left. Now note that because D is the first to carry out a limited attack in the game, the decision of what to do after each state has attacked $M_D - 1$ times belongs to D. But because C only has p_D left to lose, an attack by D will leave C with nothing left to lose. Thus, if D attacks for the M_Dth time, C will be left with nothing more to lose, and the game will end in both states' complete destruction. Therefore, if $M_C \geq M_D$, the last decision node in the game belongs to D and is the node at which D must decide whether or not to attack for the M_Dth time. (In the game in Figure 1, $M_C \geq M_D = 4$.)

Now suppose $M_C < M_D$. After C has attacked $M_C - 1$ times and D has attacked M_C times, D will have $r_D - (M_C - 1)p_C = p_C$ left to lose. Moreover, C must decide at this point whether or not to launch a limited attack for the M_Cth time. If it does, D will be left with nothing more to lose and the game will end in both states' utter devastation. Accordingly, the last decision node in a game where $M_C < M_D$ belongs to C and is the node at which C must decide whether or not to attack for the M_Cth time. Given all of this, the following proposition holds:

PROPOSITION 1. *The complete information game has a unique sequential equilibrium. If $M_C \geq M_D$, C challenges the status quo in this equilibrium and D submits immediately. If $M_D > M_C$, C does not challenge the status quo.*

Proof. The complete information game has perfect information, so it will suffice to specify the states' strategies. This is done through backwards programming. Suppose $M_C \geq M_D$. Then the last decision node in the tree belongs to D; that is, $\Omega_D(M_D)$ is the last decision node in the tree. If D attacks here, the game ends and D's payoff is zero. If D submits, its payoff will be positive. More precisely, D and C must have attacked each other $M_D - 1$ times for the game to have reached $\Omega_D(M_D)$. Hence, D's payoff will be $[r_D - (M_D - 1)p_C][1 - (s_D/r_D)] > 0$. Submitting at $\Omega_D(M_D)$ is D's best strategy.

The node immediately preceding $\Omega_D(M_D)$, C's last decision node, is $\Omega_C(M_D - 1)$. If C launches an unlimited attack, it receives 0. If it submits, it will do better, obtaining a positive payoff of $p_C[1 - (s_C/r_C)]$. But if C attacks, D will quit at $\Omega_D(M_D)$ without inflicting any more punishment on C. This will leave C with $p_C[1 + (w_C/r_C)$. Because $p_C[1 + (w_C/r_C)] > p_C[1 - (s_C/r_C)]$, attacking at $\Omega_C(M_D - 1)$ offers C the highest payoff.

The node immediately preceding $\Omega_C(M_D - 1)$ is $\Omega_D(M_D - 1)$. If D launches an unlimited attack here, its payoff is 0. If D launches a limited attack, C will retaliate with a limited attack at $\Omega_C(M_D - 1)$, after which D will submit at $\Omega_D(M_D)$. Because it is better to submit now than after having endured more punishment, D submits at $\Omega_D(M_D - 1)$.

Because D will submit at $\Omega_D(M_D - 1)$, C will escalate at $\Omega_C(M_D - 2)$, the node immediately preceding $\Omega_D(M_D - 1)$. This line of reasoning shows that C escalates at every $\Omega_C(m)$ and that D submits at every $\Omega_D(m)$. Thus, C exploits the situation at the beginning of the game, and D submits as soon as it is challenged.

If $M_D > M_C$, the last decision node in the tree belongs to C. Paralleling the argument for the case in which $M_C \geq M_D$, the owner of the last decision node in the tree quits at this node. Given that C submits at $\Omega_C(M_C)$, D's best strategy is to escalate at $\Omega_D(M_C)$, the node immediately preceding $\Omega_C(M_C)$. Given that D will escalate at $\Omega_D(M_C)$ and that C will quit at $\Omega_C(M_C)$, C's best strategy at the node just before $\Omega_D(M_C)$ is to quit. This line of reasoning shows that C quits at every $\Omega_C(m)$ and that D escalates at every $\Omega_D(m)$. Hence, there is no initial challenge. QED

Before turning to the role of counterforce options and the consequences of having smaller, less-destructive limited options, three remarks are in order. First, proposition 1 may be stated more concisely: the state with the largest M prevails. (If $M_C = M_D$, then, because D is the first to attack, C still prevails.) Second, no state ever launches an unlimited attack. Although each state throughout the confrontation has the ability to destroy its adversary with an unlimited attack, it never resorts to this. Of course, this should be expected in a sequential equilibrium, for since the nuclear revolution the threat to launch an unlimited attack has become inherently incredible.

Third, the equilibrium of the complete information game is quite insensitive to the precise specification of the states' payoffs. Essentially, the payoffs need satisfy only two conditions. First, the payoff to escalating must be sufficiently high and the cost one pays to escalate sufficiently low to induce a state to escalate if it is certain that its adversary will submit immediately thereafter. This holds in the present model because escalating by launching a limited attack is assumed to hurt the adversary but not the attacking state. Interestingly, this formal condition is quite similar to one that Kahn takes to be characteristic of the general problem of escalation: "The value of victory is usually great enough so that it would be worthwhile for either side to raise its commitment enough to win the escalation *if it were certain that the other side would not counter the rise*" (Kahn 1965, 7). The second condition required of the payoffs is that the owner of the last decision node in the tree must quit. With these two restrictions and complete information, regardless of the precise specification of the payoffs, the game unravels, just as it did in the proof of proposition 1, leaving the unique sequential equilibrium defined in that proposition.

The evolution of U.S. nuclear strategy has been characterized by a perennial call for limited options that are more selective and inflict less punishment on an adversary. There has also been a frequent call for counterforce options capable of destroying an adversary's military capabilities or disrupting its ability to carry out its operations. McNamara urged them in 1962 as part of his struggle to introduce more flexibility into the war plan or the Single Integrated Operational Plans (SIOP). These options were also at the center of the Schlesinger doctrine in the early 1970s and continued to play an important part in the countervailing strategy of the late 1970s and early 1980s (Ball 1982; Davis 1976; Freedman 1981, 1986; Sagan 1989; Slocomb 1981).

In the model each state would like to have a larger number of smaller, less-destructive limited options, as well as better counterforce capabilities, than its adversary. Because the state with the largest M prevails, a state is better off if its M is larger than its adversary's. Consider C and recall that M_C—the maximum number of C's limited attacks after which D will have nothing more to lose—is equal to r_D/p_C. If $M_C \geq M_D$, C prevails. But for any fixed M_D, M_C will be greater than or equal to M_D if p_C is sufficiently small. p_C, however, is the amount of punishment C inflicts on D when C launches a limited attack against D. In this sense, then, C is better off if it has smaller, less-destructive limited options.

C, it seems, would also like to have counterforce capabilities, because C prevails if $M_C \geq M_D$ and counterforce capabilities may reduce M_D. With $M_D = r_C/p_D$, M_D can be made less than any fixed M_C by taking p_D to be sufficiently large. But recall that p_D is the amount of punishment D inflicts on C when D carries out a limited attack. This suggests that C would be better off if it had counterforce capabilities that disrupt D's strategic forces and compel D to choose between backing down or launching larger, more-destructive limited attacks; that is, C would like to have counterforce capabilities capable of increasing p_D.

An extreme example illustrates the underlying logic. Suppose that C could launch a counterforce attack that left D with only two options: to submit or to launch a massive, unlimited attack against C. In effect, D has been put in the position of having to rely on the inherently incredible doctrine of massive retaliation and so submits. In sum, counterforce capabilities are desirable in the model, but not for the purpose of limiting damage. In the model each state can always be destroyed if its adversary decides to launch an unlimited attack. Rather, counterforce capabilities are desired for coercive bargaining purposes because they can be used to limit an adversary's options.[4]

Jervis argues that the desire for nuclear counterforce capabilities in U.S. strategic doctrine is largely due to "conventionalization," that is, "the attempt to treat nuclear bombs as though they were conventional weapons, to apply the same ways of thinking to them that applied in the prenuclear era" (Jervis 1984, 56). Conventionalization in essence denies that there has been a nuclear revolution and asserts that the classical logic of war still applies—that both superpowers' having secure second-strike forces has not fundamentally changed the relation between a state's political objectives and its use or threatened use of force. Jervis may be correct in attributing to conventionalization the desire for counterforce capabilities that has characterized U.S. strategic doctrine. But the model suggests that the conclusion that the nuclear revolution deprives counterforce capabilities of any role is problematic. In the game each state is completely vulnerable to its adversary throughout the confrontation. The nuclear revolution is implicit in the model. Yet, surprisingly, counterforce capabilities continue to have a role.

Although the equilibrium of the complete information game has some interesting and suggestive properties, it lacks dynamics. In equilibrium there are no crises. There is either no challenge or if there is a challenge, it is not resisted. To make the model dynamic, I introduce incomplete information.

Incomplete Information

A *sequential crisis equilibrium* is a sequential equilibrium in which there is some chance that there will be a crisis. Following Snyder and Diesing's (1977, 13) notion that for there to be a crisis there must be a resisted challenge, a sequential crisis equilibrium is, more formally, a sequential equilibrium in which there is a positive probability that *C* will challenge the status quo and that *D* will resist by escalating at least once.

Suppose that $M_C \geq M_D$. If there were complete information, then by proposition 1, *C* would challenge the status quo and *D* would immediately submit. But suppose that *C* is uncertain about the nature or type of its adversary. *C* believes that there is some chance that it is facing not *D* but *D'*. What distinguishes *D'* from *D* is that *D'* is wedded to a specific strategy. In particular, if *C* launches a limited attack against *D'*, *D'* will always and unquestionably retaliate by launching a limited attack against *C*. Because *D'* always retaliates and *D* would never retaliate if there were complete information, *D'* and *D* will be called the resolute and irresolute defender, respectively.[5]

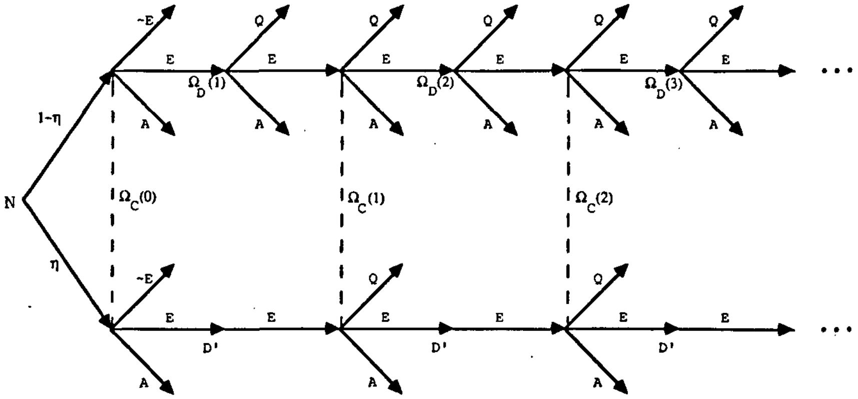
Because the game ends as soon as one of the states stops escalating, *D'*'s strategy is in effect "tit-for-tat." As long as *C* escalates, so will *D'* and the escalation cannot end until *C* "cooperates" by not escalating. Tit-for-tat has recently received a good deal of attention (e.g., Axelrod 1984), and modeling the lack of complete information by assuming that *D'* plays this strategy may contribute to developing a deeper understanding of its role in crisis bargaining. Another reason for treating *D'* in this way is that *D'* is in a certain sense irrational. It will continue to escalate even if that ultimately means leaving *C* with nothing more to lose and thereby bringing about its own destruction.

C, then, is uncertain whether it is facing a completely rational adversary, represented by *D*, or a somewhat irrational adversary like *D'*. This in turn gives *D* an opportunity to exploit the strategy of the "rationality of the irrational" (Kahn 1965, 57–58); that is, *D* can attempt to play on *C*'s doubts about its adversary's rationality by trying to build and maintain a reputation for being irrational. Thus by modeling *D'* in this way, the strategy of the rationality of the irrationality may also be examined. (A third reason for treating *D'* in this manner is that it greatly simplifies the game tree and subsequent analysis.)

C's uncertainty about the nature of its adversary creates a dilemma. If *C* were certain that it was facing *D*, it would challenge the status quo. But if *C* were certain that it was facing *D'*, it would never dispute the status quo. If it did, *D'* would retaliate and continue to do so until *C* backed down or until both states had bled themselves to death. The fact that *C* will back down if it is sufficiently confident that it is facing *D'* gives *D* an incentive to develop and maintain a reputation for being resolute. *C*'s struggle to determine if its adversary is actually resolute and *D*'s struggle to maintain a reputation for being resolute and derive the benefits of it are the seeds of escalation.

To simplify the discussion, the analyses will focus on the case in which $M_C \geq M_D$ and there is only one-sided incomplete information. The incomplete information game is illustrated in Figure 2. With $M_C \geq M_D$, the last information set belongs to *D* and is $\Omega_D(M_D)$. The one-sided incomplete information will be modeled by assuming that only the challenger is uncertain of the type of its adversary. *C* believes that it is facing the irresolute defender *D* with probability $1 - \eta$ and the resolute defender *D'* with probability η where η is common knowledge. In effect, the game

Figure 2. Limited Retaliation with Incomplete Information



begins with a random move that decides the type of the defender, D with probability $1 - \eta$ and D' with probability η. Let $\beta_C(m)$ denote the probability that C is facing D at $\Omega_C(m)$; that is, C believes that it is facing D with probability $\beta_C(m)$ at $\Omega_C(m)$. Accordingly, $\beta_C(1) = 1 - \eta$, that is, C initially believes that its adversary is D with probability $1 - \eta$. Then the following proposition holds.

PROPOSITION 2. *In all of the noncrisis sequential equilibria of the incomplete information game, the challenger does not dispute the status quo. If, moreover, $\eta > \bar{\eta}$ ($\bar{\eta}$ is defined below), there are no sequential crisis equilibria. If $\eta < \bar{\eta}$, there is an essentially unique sequential crisis equilibrium. D's strategies and beliefs are given by*

$$e^*_D(1) = \eta(r_C - s_C)/((1 - \eta)\{(M_D - 1)(w_C + s_C) + e^*_D(2)[r_C - s_C + (M_D - 1)(w_C + s_C)]\})$$

For $2 \leq m \leq M_D - 1$,

$$e^*_D(m) = [M_D - (m - 1)](w_C + s_C)/\{[M_D - (m - 1)](w_C + s_C) + [1 - e^*_D(m + 1)][r_C - s_C + (M_D - m)(w_C + s_C)]\}.$$

$$e^*_D(M_D) = 0.$$

And for all m, $a^*_D(m) = 0$. C's strategies and beliefs are given by $e^*_C(0) = 1$. For $1 \leq m \leq M_D - 1$,

$$e^*_C(m) = [M_C - (m - 1)](w_D + s_D)$$
$$\{r_D - s_D + [M_C - (m - 1)](w_D + s_D)\}$$

and for all m,

$$a^*_C(m) = 0$$

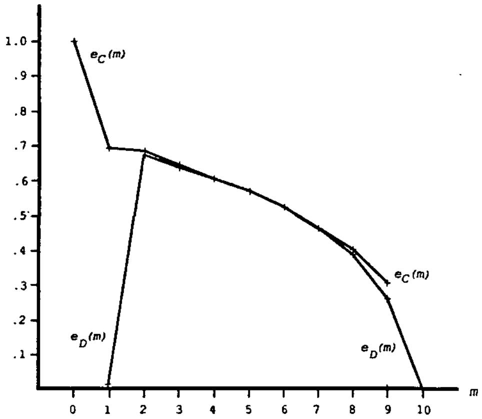
$$\beta^*_C(m) = (1 - \eta)[\prod_{i=1}^{m} e^*_D(i)]$$
$$\{\eta + (1 - \eta)[\prod_{i=1}^{m} e^*_D(i)]\}^{-1};$$

and

$$\bar{\eta} = M_D w_C\{(M_D - 1)(w_C + s_C) - e^*_D(2)[r_C - s_C + (M_D - 1)(w_C + s_C)]\}/\{[r_C - s_C + M_D(w_C + s_C)][1 - e^*_D(2)][r_C - s_C + (M_D - 1)(w_C + s_C)]\}$$

Powell (n.d.) derives the equilibrium strategies and beliefs in the more general case in which there is two-sided incomplete information. Accordingly, only a very brief sketch of the derivation of proposition 2 will be given here. The first

Figure 3. The Dynamics of Escalation



step is to show that in a sequential crisis equilibrium, $0 < e^*_C(m) < 1$ for $1 \le m \le M_D - 1$. This implies that at any information set $\Omega_C(m)$ for $1 \le m \le M_D - 1$, C is indifferent between submitting and escalating; that is, the payoff to submitting equals the payoff to escalating. This fact and some tedious algebra gives the expressions for the equilibrium strategies. Finally, ensuring that all of the strategies are feasible, that is, that all of the probabilities are between zero and one, yields the condition that $\eta < \bar{\eta}$.

Proposition 2 describes the dynamics of escalation in the strategy of limited retaliation. The challenger initially believes that it is facing an irresolute defender with probability $1 - \eta$. If this probability is sufficiently high, the challenger is confident enough that its adversary is irresolute and challenges the status quo. This shifts the onus of escalation to the defender, which now escalates by launching a limited attack with probability $e^*_D(1)$. The defender's retaliation now shifts the onus of escalation back to the challenger, leaving it less confident that the defender is irresolute. The challenger is now less likely to escalate: $e^*_C(1) > e^*_C(2)$. Indeed as the confrontation continues and the onus of escalation shifts back and forth, C becomes less and less confident its adversary is irresolute and less and less likely to escalate: $\beta^*_C(m) > \beta^*_C(m + 1)$ and $e^*_C(m) > e^*_C(m + 1)$. Similarly, the defender also becomes less and less likely to escalate: $e^*_D(m) > e^*_D(m + 1)$ for $2 \le m \le M_D - 1$. Figure 3 illustrates the dynamics of escalation for the case in which $M_C = M_D = 10$; $r_C = r_D = 10$; $w_C = w_D = 1$; $s_C = s_D = 1$; and $\eta = .00001$.

Crisis Stability and Limited Options

Underlying the perennial call in U.S. strategic nuclear doctrine for having smaller, less-destructive limited options is the belief that deterrence depends on credibility and that credibility in turn depends on the likelihood that a state would actually carry out its threat if ever called upon to do so. Presumably a threat to carry out a smaller, less-destructive limited attack would be more credible and thereby enhance deterrence by making crises less likely (Greenwood and Nacht 1974; Osgood 1979). But these threats are more credible precisely because they are more likely to be carried out. Even if having less-destructive options enhances deterrence by making a challenge to the status quo less likely and thereby reducing the chances of a nuclear exchange, this reduction may be offset by the fact that these weapons are more likely to be used should the status quo be challenged. The net effect may be to make a nuclear exchange more likely and in this sense reduce crisis stability (Panofsky 1973; Scoville 1974). The model provides a way of very tentatively balancing these opposing influences.

If the defender has smaller, less-destructive limited options, this may be modeled by reducing p_D, the amount of destruction wrought by D if it escalates. As p_D decreases, M_D, which is equal to r_C/p_D, increases. Recall that the model is based on the assumption that $M_C \geq M_D$. If, therefore, M_D increases enough that $M_D > M_C$, C will not dispute the status quo. This follows immediately by noting that if $M_D > M_C$, the last decision node in the tree now belongs to C. The argument used to prove proposition 1 then applies to the incomplete information game and shows that C will never challenge the status quo. With $M_D > M_C$ there will be no challenges and the probability of a nuclear exchange is zero. On balance, the effect of having options that are sufficiently small and limited that $M_D > M_C$ is to reduce the chances of a nuclear exchange.

Now suppose that the defender has less-destructive limited options but that $M_C \geq M_D$ and $\eta < \bar{\eta}$ so that there remains some chance of a challenge to the status quo. The probability that D will resist a challenge, $e^*_D(1)$, is increasing in M_D. Thus, as long as $M_C \geq M_D$ and $\eta < \bar{\eta}$, the probability of there being a nuclear exchange, $e^*_C(0)e^*_D(1)$, increases as the defender acquires smaller, less-destructive limited options. In this case these options reduce crisis stability. A large reduction in the destructiveness of the defender's limited options may make the probability of a nuclear exchange smaller. A smaller reduction in the destructiveness of these options, however, may reduce crisis stability and make a nuclear exchange more likely.

Conclusions

Although mutually assured destruction may be the technological state of affairs, there are still several conceptual approaches to nuclear deterrence. Recent formal work has focused on brinkmanship crises in which states exert coercive pressure by manipulating the risk of an unlimited nuclear exchange. I have attempted to extend the formal analysis of nuclear deterrence theory to the strategy of limited retaliation in which states exert coercive pressure by inflicting limited amounts of damage on an adversary in order to make the threat of future punishment more credible.

The strategy of limited retaliation has been modeled as a simple game of sequential bargaining. The game's equilibrium suggests that states prefer to have relatively smaller, less-destructive limited options. Moreover, each state also finds counterforce options desirable even though these options are incapable of limiting the total amount of damage an adversary can inflict on the state. When one-sided incomplete information is added to the model, the escalatory dynamic described by the game's unique sequential crisis equilibrium shows that as the crisis continues, the states become less and less likely to escalate further. The challenger also becomes less and less confident that it is facing an irresolute adversary. Moreover, a large reduction in the destructiveness of the defender's limited options may make the probability of a nuclear exchange smaller. A smaller reduction in the destructiveness of these options, however, may reduce crisis stability and make a nuclear exchange more likely. Finally, uncertainty and incomplete information play a crucial role in enhancing deterrence. Doubts about the defender's resolve may deter a state from making a challenge that it would have been certain to make had it been completely confident about the defender's resolve.

Notes

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1. This is actually the caricature of the doctrine that the doctrine's critics attacked. The actual doctrine was more complicated and subtle. See, for example, Freedman 1986 and Wells 1981.

2. See Jervis 1984 for a summary and harsh cri-

tique of escalation dominance and the countervailing strategy.

3. See Kreps and Wilson 1982 for a formal definition of a sequential equilibrium and a discussion of its properties. See Powell 1987 for a discussion of the use of sequential equilibria to solve the credibility problem in the context of nuclear brinkmanship.

4. The conclusions about the desirability of having smaller, less-destructive limited options and of having counterforce capabilities must be interpreted cautiously. A state is clearly better off in the model, the larger its M, and in this sense it prefers to have the options that make M relatively larger. But these conclusions would be much stronger if the destructiveness of the options and the ability to procure various levels of counterforce capabilities were endogenous in a more refined model and if in the equilibria of this refined game the states actually decided to have relatively less-destructive limited options and to have counterforce capabilities.

5. Introducing incomplete information by assuming that D' has a single strategy greatly simplifies the game and the problem of finding the game's equilibria. Kreps and colleagues (1982) use this approach to study rational cooperation in a finitely repeated prisoner's dilemma.

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THE POLITICAL ECONOMY OF MILITARY SPENDING IN ISRAEL

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Prior scholarly analysis of Israeli military spending has focused on national security questions. We present a mathematical model incorporating security threats as well as electoral cycles and corporate profits. The parameters are estimated empirically. The results support the idea that in Israel the military budget at the margins is also *employed as a political-economic instrument to help manage the economy and to provide a favorable election climate for incumbents. It is suggested that the political-economic dynamic widely attributed to Western industrialized societies may be of increasing importance in other societies throughout the world.*

Defense expenditures in the Third World grew from $104.2 billion in 1972 to $180 billion in 1982 (U.S. Arms Control and Disarmament Agency 1984, 11). Such rapid growth is clearly a by-product not only of security considerations but also of domestic political and economic influences. Evidence from research on the U.S. case (Fischer and Crecine 1979; Nincic and Cusack 1979; Ostrom 1977) and the Soviet case (Nincic 1982), as well as research on some West European countries (Domke, Eichenberg, and Kelleher 1983; Eichenberg 1984) points to the importance of domestic constraints in shaping armament spending policies. "Linkage" variables such as political parties (Bishop and Sorenson 1982), electoral cycles (Nincic and Cusack 1979), public opinion blocs (Russett and DeLuca 1981), organized labor unemployment (Griffin, Devine, and Wallace 1982), and monopoly corporate profits (Mintz and Hicks 1984) are frequently cited as shaping defense expenditures in the developed world. Yet these forces have rarely been examined empirically with respect to the military-spending behaviors of other countries (e.g., Chatterji 1969; Hollist 1977; Mintz 1986). We assess the impact of several of these variables on the evolution of Israeli defense expenditures on the 1960–84 period.

We choose Israel for several reasons. The involvement of Israel in the 40-year Middle Eastern arms race, the participation of the superpowers in this competition, and Arab-Israeli tensions have all served to highlight the importance of Israeli military spending. Second, Israel has developed a sizable, potent domestic arms industry, governed by large state enterprises. Since the arms industry is sizable, a greater potential exists for policy impact to result from manipulation of military spending patterns. Over the past two decades the arms industry has been a crucial component influencing the economic development of Israel.[1] Third, most Israelis believe that questions of military spending are *apolitical,* owing to the perceived threat to national security (Lissak 1983). Further, Israelis are constantly being asked to make societal sacrifices to finance the defense budget. Israel consistently ranks among the top in military expenditures per capita (U.S. Arms Control and Disarmament Agency 1984). Israel has also relied on massive military and economic aid infusions from the United States. Consequently, the normative and political implications of the nonmilitary uses of the Israeli defense budget are enormous. All these reasons together make Israel an especially good case for testing the ideas presented in this research.

Other societies with sizable and growing defense industries, such as Brazil, Taiwan, South Korea, Pakistan, and Egypt also devote between 25% and 50% of government expenditures to the military (U.S. Arms Control and Disarmament agency 1984). Accordingly, with appropriate modifications, the general thesis may also apply to some of these countries.

The Political Economy of Military Spending

Since the mid-1960s, arms production in the Third World has expanded rapidly in both scope and extent. Twenty-six countries are now producing arms in the Third World, of which six possess so-called across-the-board capabilities; that is, they can produce all categories of arms (Neuman 1984). The proliferation of coproduction and licensing agreements from the North to the South now enables many countries to manufacture or assemble weapons systems domestically, including some of the most sophisticated systems available (Pierre 1981–82, 267). In the Middle East, Egypt is currently producing the Alpha jet aircraft under license from France and the AN-TPS-63 radar with U.S. assistance (Klare 1983, 72). Saudi Arabia is being assisted by the United States in modernizing its armed forces. Iraq is producing small arms, and Syria is manufacturing ammunition (Brzoska and Ohlson 1986).

This proliferation of military weapon systems produced in the Third World has led to growing economic dependence of some of these countries on indigenous arms production and consequently represents a growing potential for political manipulation of domestic defense budgets. Policy makers realize that military expenditures and military aid funds can also be used as a pump-priming mechanism in managing the domestic political economy. Specifically, such quantities of resources can be employed to stimulate sluggish corporate profits, enhance political support before an election, and manipulate voters in anticipation of elections in the donor country.

Many Third World countries are involved in intensive regional conflicts that often entail arms races. Consequently, the use of resources specifically earmarked for military purposes to improve the economic well-being of military contractors or enhance the political fortunes of leaders is controversial. It almost goes without saying that the standard of living of many of these societies is lower than that of more developed countries. Further, income inequalities are often severe. Since there is no reason to assume that military spending is the most useful mechanism for raising economic standards, the defense-welfare trade-off is particularly stark. Nonetheless, we suggest that domestic political and economic considerations are so dominant in the calculus of decision makers that they influence military expenditures, at least at the margins. Consequently, we hypothesize that declining corporate profits lead to increased military spending, that electoral periods are associated with increased military spending, and that elections in the donor country are associated with changes in military aid levels. These forces should be visible in Israel.

The Political Economic Context

Arms production activity in Israel accounts for a very large share of overall industrial activity (Brzoska and Ohlson 1986, 22). Specifically, investment in the defense sector accounts for as much as 50% of all industrial investment in Israel. Almost one-fourth of the industrial labor force in Israel is engaged in military-related projects, about one-fourth of Israel's industrial exports (excluding diamonds) originate in the defense sector, and 8 of the 20 largest corporations in Israel are highly dependent on military contracting. Three of the top five Israeli companies are primarily concerned with production of goods and services for the defense sector (Mintz 1987).

Manufacturing and services of defense items are significant in terms of employment, revenues, and exports in all three sectors in the Israeli economy. These influences range at the high end from the government's commercial enterprises, through those of the Labor Federation, to a slightly smaller weight in the purely private sector (Mintz 1987, chap. 9). The level of industrial dependence on defense contracting in Israel created a situation whereby the Israeli government has been forced in recent years to consider not only the security ramifications of its military procurement policies but also the economic and political implications of arms production decisions. This was evident in the decision to develop the Lavi aircraft despite opposition from the ground forces of the Israeli military and later on to cancel it despite support for the project from the commander of the air force. Similar concerns were visible in numerous decisions about the exportation of military equipment from Israel. The oligopolistic structure of the defense industry in Israel is reflected in the fact that six major companies dominate the market. The dependence of the government on these major concerns for economic growth, revenues, jobs, and exports, has made the government sensitive to the economic well-being of these companies. Bichler (1986) notes that military expenditures are essential for the economic well-being of major concerns in Israel. Steinberg and Hadar (1988) similarly concluded that the largest concerns are the main beneficiaries of governmental support for high-tech projects, such as characterize much defense spending. Consequently, military expenditures are expected to increase significantly following a decline in profits of the major industrial concerns in Israel.

Given the fragile nature of coalition politics in Israel, where two major and roughly equal bodies—the Labor and the Likud parties—are struggling to achieve power, all recent Israeli elections are highly competitive, and incumbents may be tempted to manipulate even the putatively apolitical defense budget. Moreover, as the Israeli electorate shifts more intensively toward the Right, as reflected in elections 1973, 1977, and 1981, neither party can allow itself to appear unresponsive and "unpatriotic" towards defense spending (Mintz 1988). The pervasive influence of military spending makes it difficult for elected politicians to vote against increases in the budget. We hypothesize that defense expenditures grow before an Israeli election. Since domestic political considerations also influence U.S. aid decisions (Bard 1988; Quandt 1988), we hypothesize that changes in U.S. aid to Israel also reflect the electoral cycle in the United States. These political and economic considerations are vastly overshadowed by Israel's geopolitical and strategic situation stemming from regional security concerns. In fact, some would argue that if there is any state where the defense budget is unlikely to be used as a political and economic management tool, it is Israel, since Israel is surrounded by real enemies with which it has fought real wars.

The Security Context

The arms race between the Arab states and Israel has been a fact of life in the Middle East for several decades. The race gained momentum following the Yom Kippur War of 1973. In part this was due to the surplus petrodollars that resulted from the rapid rise in world oil prices. This arms race continued throughout the 1980s. The quantities of weapons transferred to the region were matched by qualitative changes as well. Consequently, both Israel and its rivals accumulated large quantities of sophisticated conventional weaponry (Heller, Tamari, and Eytan 1983). The race has come at a staggering cost. Middle Eastern countries are currently buying more than $15 billion annually in foreign armaments—"more than all NATO and Soviet bloc countries combined"—and military spending from the midseventies to the mideighties totaled more than $500 million (Congressional Quarterly 1986, 67).

The Arab-Israeli arms race is often viewed from the perspective of the ongoing regional conflict. This conflict has had three phases in its development: "The first was the civil struggle between Arab and Jewish Palestinians during the British Mandate period. The second began in 1948 when Israel declared its independence and the Arab states became involved, engaging Israel in its first war. The third stage, harder to pinpoint, came some time after a later war, in 1973. It was then that the conflict focus shifted from the Arab states to the Palestinians, particularly the Palestine Liberation Organization. This shift was strengthened in 1979 when Egypt signed a peace treaty with Israel" (Congressional Quarterly 1986, 7).

In general it is the case that Arab and Israeli military spending trends move up and down in symmetry. Based on Stockholm International Peace Research Institute data (1987, 234), six out of eight years between 1976 and 1985 show a correspondence in the direction of percentage increases between Israel and Egypt, Jordan, and Syria. Consequently, when the military spending of the confrontation states is going up, Israeli military budgets also tend to go up. Similarly, they both tend to be reduced in the same years. Figure 1 shows the individual expenditure figures for Israel, Egypt, Jordan, and Syria. These data illustrate that even in periods of drastic developments in the Middle East (the Egyptian-Israeli peace treaty, the war in Lebanon), changes in Israeli defense expenditures correspond to changes in Arab military expenditures. In particular, one observes that until the Six Day War in 1967, Egypt outstrips Israel's military spending, although by decreasing amounts. Subsequently, both Israel and Egypt continued to accelerate their military spending, though Egypt's spending was lower than Israel's. Additionally, during this period (1967–73) Syria began rapidly increasing its military outlays. Following the 1973 war, there was additional upward pressure on military expenditures. During the post–Camp David period Egyptian military spending is markedly reduced, Israel exhibits a level trajectory of real military spending, and Syria climbs to a new high point, then levels off. It appears that there is a relative stability in the overall spending patterns during the 1980s. It is worth noting that in combination Egypt, Syria, and Jordan outspend Israel. The security threat, the Middle Eastern arms race, and the Arab-Israeli wars are taken for granted as factors shaping the evolution of Israel's military expenditures.

A Mathematical Model

To test the theoretical argument about the impact of corporate profits, electoral cycles in Israel and the United States, Arab military spending, and the Middle Eastern wars on Israeli defense expenditures, we specified and empirically estimated a model as a system of dynamic, nonlinear, differential equations.

The point of departure is the assumption that the Israeli military budget consists of two sets of expenditures: domestic defense expenditures, which normally account for at least 50% of the total budget, and military imports, which constitute the bulk of the remaining items. The former is primarily earmarked for such budget items as compensation of employees in the defense sector, purchases and services from the domestic economy, and construction; while the latter consists mainly of imports of weapon systems. Together, these components represent the total Israeli military budget.

Previous studies showed that the Arab-Israeli wars, the military expenditures of the Arab states, and the Israeli Defense Forces (IDF) intensification programs strongly affected both the domestic defense budget and military imports. We hypothesize that domestic political-economic factors, too, influence spending, when declining corporate profits and electoral politics strongly affect the domestic defense budget, and that electoral cycles in the United States significantly influence U.S. aid decisions, which in turn affect Israel's military imports directly.

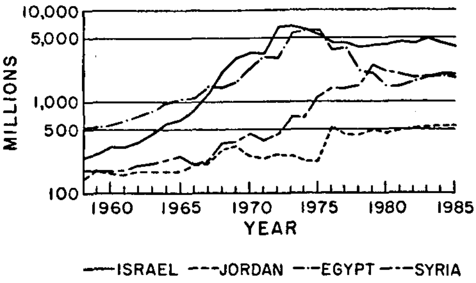
Model Specification

The first part of the model focuses on the domestic defense budget. This component of military spending is represented in equations 1 and 2:

$$\tilde{y}_1^s = \beta_1 y_3^s + \beta_2 x_1 + \beta_3 x_2 \qquad (1)$$

$$\frac{dy_1^s}{dt} = \beta_4 \{\tilde{y}_1^s - y_1^s\} + \beta_5 x_3 + \beta_6 x_4 - \beta_7 x_5 \qquad (2)$$

Figure 1. Arab and Israeli Military Spending Patterns, 1958–85 (Million 1980 U.S. Dollars, Logarithmic Scale)



By way of notation, y_i variables are endogenous, while x_i variables are exogenous. Inferred target variables, which are not measured, are indicated by a tilde, as in $\tilde{y}_i$. Endogenous variables measured in shekels are flagged with a superscript *s*, those measured in U.S. dollars with a *d:* y_i^s or y_i^d, respectively.

The first of these equations portrays the domestic budget requests, or the desired level of domestic spending ($\tilde{y}_1^s$), being driven by three forces: defense imports (y_3), war involvement (x_1), and the percentage increase in Egyptian, Syrian, and Jordanian military expenditures (x_2). Defense imports, consisting mainly of procurement of new military equipment, influence the domestic defense budget, since the acquisition of new military equipment normally requires the expansion of maintenance and operations programs. Consequently, defense imports tend to place an upward pressure on the domestic defense budget and do not, we hypothesize, involve trade-offs of the kind McGuire (1982) has identified in other aspects of Israeli resource allocation related to defense policy making. During periods of war involvement in which a high number of Israeli casualties occur, additional pressure is placed on defense needs, since war involvement (x_1) also invariably leads to replacement of military equipment that is destroyed or shown to be obsolete on the battlefield. These pressures lead to increases in the budgetary request. Finally, no model of contemporary and historical defense spending in Israel is complete without inclusion of some direct measure of the impact of the perceived threat that Israeli decision makers face. To the extent that the Arab states are increasing their military expenditures, Israeli defense planners feel compelled to request increases in the resources allocated to the domestic defense budget.

Equation 1 portrays the hypothesized relationship between the military budget requests and three, linear, additive components: defense imports, war involvement, and changes in Arab military expenditures. It represents the desired level, or goal, for spending on the domestic aspects of the military budget in Israel. Equation 1 is used to create a measured goal, which itself is employed in equation 2.

Equation 2 describes the adjustments that we hypothesize policy makers make in the domestic budget requests in response to political and economic pressures. The primary mechanism is an adaptive one that drives expenditures (y_1^s) toward the requests as defined in equation 1, namely, $\tilde{y}_1^s$. The rate at which the actual and the desired converge is captured in the adjustment coefficient β_4. Due to the security threat to Israel and the strength of the Israeli military elite, we hypothesize that the level of defense expenditures approved adapts to the requested level of military expenditures.

The budgetary requests are adjusted upward due to domestic pressures. In periods of declining corporate profits (x_5) for example, the government may use defense spending as a countercyclical economic device to stimulate the economy, prevent unemployment, and increase profitability of the defense industry. Accordingly, we hypothesize that it exerts a negative influence. Moreover, while the rate of growth in compensation of employees in the military sector in the year preceding an election is typically smaller than normal (Mintz 1988), the *total* amount of spending is thought to be gradually adjusted upward as elections (x_4) approach, since it is politically beneficial for incumbents to create the illusion of a flourishing economy just before an election and since politicians are unable to say *no* to demands for increased spending before an election. Inflation (x_3), as captured by the consumer price index, has also been shown to accelerate defense spending (Ward and Mintz 1987).

The second part of the model focuses on Israel's military imports. This component of military spending is represented in equations 3–6. These equations portray the level of military imports that Israel receives, valued in constant dollars (y_2^d), as being driven by three different forces, most importantly, the amount of military aid Israel receives (Berglas 1983).

$$\tilde{y}_2^d = \beta_8 x_2 + \beta_9 x_6 \qquad (3)$$

$$\frac{dy_2^d}{dt} = [\beta_{10}(\tilde{y}_2^d - y_2^d) + \beta_{11} x_7] s_1 \qquad (4)$$

$$\frac{dy_3^s}{dt} = [\beta_{12}(y_2^s - y_3^s)] s_1 + [\beta_{13}(\tilde{y}_3^s - y_3^s)](1 - s_1) \qquad (5)$$

$$\tilde{y}_3^s = \beta_{14} x_2 + \beta_{15} x_6 \qquad (6)$$

Equation 3 represents the establishment of a target for Israeli military aid ($\tilde{y}_2^d$). It should be stressed that this equation represents some consensus between the United States and Israel as to the needs of the Israelis. We posit that there is broad agreement on (1) the military threat of the so-called confrontation states (Egypt, Jordan, and Syria), herein reflected in x_2 and (2) the military needs of the Israel Defense Forces, as measured by the intensification programs of the IDF (x_6). While defense expenditures of the Arab countries influence Israel's military expenditures, a reciprocal influence is not ruled out (see Hollist 1977 for such evidence).

The Israeli military aid target ($\tilde{y}_2^d$), as negotiated between the United States and Israel, accordingly responds to changes in Arab military expenditures (x_2), and major intensification programs (x_6). The effect of war involvement on the Israeli military aid target is excluded from this equation, since it is captured independently by the Arab postwar intensification efforts and the IDF intensification programs. The correlation Israeli war involvement and Arab military expenditures is .70. Luttwak (1984, 131) argues that Israel's weapon system intensification programs have reflected the force structure goals of the Israeli armed forces, as derived from "evolving operation concepts of war as well as changing definitions of national security." This stands in contrast to many Third World nations that tend to define their defense needs in retroactive response to their weapons suppliers. Nevertheless, as in other countries, the military aid target in Israel is virtually always greater than the amount actually granted. Equation 4 portrays the evolution of actual military aid (y_2^d) as it adjusts to domestic political constraints in the United States. Consequently, we hypothesize that the timing of increases in aid to Israel corresponds to the electoral cycle in the United States (x_7), as well as to the desired aid target ($\tilde{y}_2^d$).

Finally, we posit that military imports (equation 5) adjust to the amount of military aid granted by the United States. In earlier periods in which there was no military aid granted by the United States, military imports adjusted to the percentage changes in Arab military intensification programs as reflected in their defense spending and to IDF intensification programs. Accordingly, we posit different mechanisms for post-1967 imports (the first term in the equation) and pre-1967 military imports (reflected in the second term in the equation). The variable s_1 is a switch between each of these two different aid regimes. Equation 6 simply captures the pre-1967 goal for military imports ($\tilde{y}_3^s$) as a function of Arab military spending (x_2) and IDF building programs (x_6).

To represent total Israeli defense expenditures an identity is used: the sum of domestic expenditures plus defense imports. This identity is represented as equation 7 in the mathematical model:

$$y_4^s \equiv y_1^s + y_3^s \tag{7}$$

Data Sources and Estimation Methodology

The basic procedure employed nonlinear minimization of a goal function defined for the entire system of equations. This goal function is minimized subject to the parameters of the system, which are constrained to be positive. The methodology is explained in some detail in Ward 1984 and has been employed in a wide variety of studies in political science. (See Allan 1983; Lambelet, Luterbacher, and Allan 1979; and Luterbacher and Allan 1982 for single equation versions of the approach. Ward 1984, Ward and Mahajan 1985, Ward and Mintz 1987, and many macroeconomic studies such as Gandolfo 1981 and Wymer 1972, among others, employ a system approach to minimization.)

The specific strategy employed consists of first obtaining single-equation nonlinear estimates of the parameters to provide initial values for the system estimation. The estimated single-equation parameters then become starting values with which to begin the minimization of the system criteria. The system criteria, or R^2_{sys} is given by

$$R^2_{\text{sys}} = 1.0 - \frac{det(e'e)}{det(y'y)}$$

Table 1. Estimated Parameters and Standard Errors

Parameter	Parameter Description	Estimated Value	Standard Error
Equations 1–2[a]	domestic military spending		
β_1	feedback of defense imports	.0	1.99
β_2	impact of war casualties	30.07	1.17
β_3	impact of Arab military spending	736.79	28.08
β_4	adjustment speed	.00294	.00006
β_5	impact of inflation	.0	.0004
β_6	impact of Israeli election	46.28	3.18
β_7	impact of corporate profits	.0113	.0056
Equations 3–4[b]	U.S. military aid to Israel		
β_8	impact of Arab military spending	114.8	5.7
β_9	impact of IDF programs	.0	123.7
β_{10}	adjustment speed	.084	.005
β_{11}	impact of U.S. election	226.0	17.8
Equations 5–6[c]	Israeli defense imports		
β_{12}	adjustment speed, post-1967	1.53	.59
β_{13}	adjustment speed, pre-1967	.00895	.00186
β_{14}	impact of Arab military spending, pre-1967	235.4	96.8
β_{15}	impact of Arab military spending, post-1967	8,466	2,852

Note: $\bar{R}^2_{\text{sys}} = .964$.

[a] Dependent variable = y_1^s; $\bar{R}^2 = .973$; standard error = 74.8.

[b] Dependent variable = y_2^d; $\bar{R}^2 = .674$; standard error = 294.

[c] Dependent variable = y_3^s; $\bar{R}^2 = .799$; standard error = 156.

such that *y* is a matrix of observations on the *G* endogenous variables, and *e* is a matrix of [*G*, *N*] errors. This measure is similar to the standard R-squared measure and has a comparable interpretation, but it is not an average of individual equation R-squareds, since it takes into account the overidentifying restrictions. Anderson (1958, 166–69) provides an early and clear statement of this. An excellent, up-to-date text on nonlinear statistical models is Gallant 1987.

To estimate the model, data were taken from official Israeli government statistics wherever possible. The data on domestic defense expenditures, defense imports, and total military expenditures (all in constant 1975 Israeli shekels) represent final outlays and were obtained from the Israeli Central Bureau of Statistics.[2] Data on military aid to Israel (in constant 1975 U.S. dollars) were taken from the Bank of Israel's *Annual Report*. The gross domestic product deflator, annual inflation index, and exchange rates were taken from publications of the International Monetary Fund,[3] while figures on the annual percentage change in Egyptian, Jordanian, and Syrian military expenditures (in constant dollars) were computed from data obtained from Stockholm International Peace Research Institute 1987. Data on corporate profits were taken from Bichler 1986, reporting percentage annual change in the profits of major economic concerns in Israel. War casualties were taken from Zussman 1983 and the Israel Information Center's *Israel Government Yearbook*. These figures refer to Israeli casualties in wars and not in other military activities such as preemptive and retaliatory attacks against the Palestine Liberation Organization. Information on the intensification programs of the IDF were taken from the Israeli Defense Forces' *IDF in Its Might* (1982) and were estimated by the authors for the years subsequent to 1982. These data refer to the introduction of new weapons systems such as the F-4, F-15, and F-16. The post–Camp David buildup in the Israeli Negev is excluded, since it is not included in the official IDF budget.

Empirical Results

Table 1 presents the results of the statistical estimation of the parameters for the mathematical model. Figures 2, 3, and 4, respectively, present the actual and fitted trajectories of the three major endogenous variables: domestic military expenditures (y_1^s), U.S. military aid to Israel (y_2^d), and Israeli foreign military imports (y_3^s). Figure 5 presents the total Israeli military budget as composed of the two estimated and actual series, namely, the domestic budget plus the foreign military imports. Each of these graphics illustrates the relatively good fit of the estimated trajectories with the actual data and suggest that the data and the model are compatible one with another.

Turning to the first component of the model, parameters β_1–β_7 and Figure 2, it is clear that the overall fit of the model and the data is strong. Not only is the level of domestic military spending adequately reflected by the model, but the major turning points in this series are also captured. Moreover, five of the seven parameters are of the expected sign and much greater than their corresponding standard errors.

The largest impact on domestic military spending comes through the monitoring of Arab military spending patterns, as seen in the large, significant, positive coefficient, β_3. In the same vein, the weight of Israeli war casualties on the domestic military budget is, as hypothesized, considerable. Thus, the security concerns of the Israelis are seen to play the major role in determining the domestic military budget in Israel. Nonetheless,

Figure 2. Actual and Predicted Israeli Domestic Defense Expenditures (1975 Shekels)

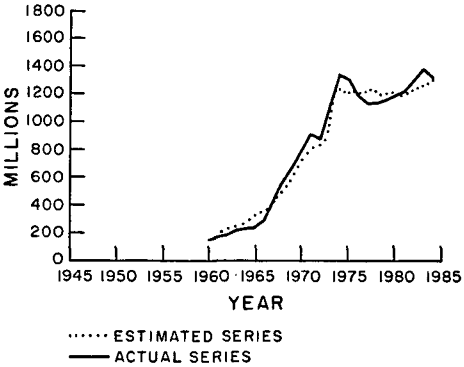


Figure 3. Actual and Predicted U.S. Military Aid to Israel (1975 U.S. Dollars)

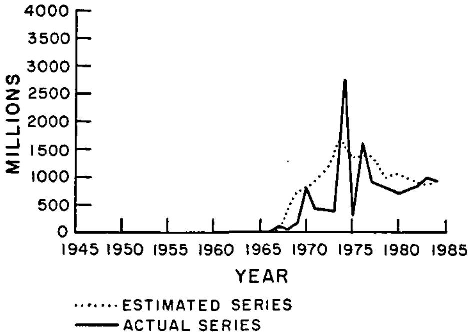
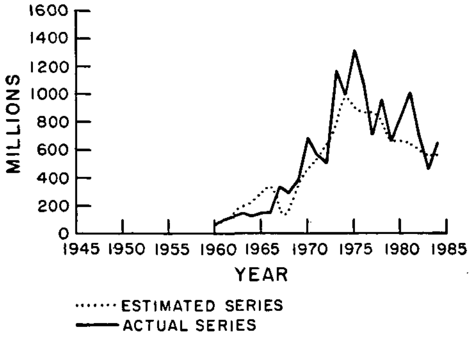


Figure 4. Actual and Predicted Israeli Defense Imports (1975 Shekels)



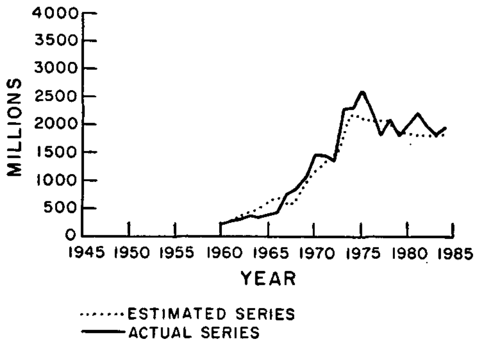
both the onset of the election period and corporate profits play important parts in the evolution of the domestic military budget, although the hypothesized feedback from Israeli defense imports (y_3^s) to the domestic side of the budget turns out to have a negligible impact, with the estimated parameter, though positive, being essentially zero and nonsignificant. Additionally, inflation was also found to have a parameter that was effectively zero and nonsignificant.

In particular, as suggested, the competitive election dynamic in Israel makes it difficult for the incumbents to "just say *no*" to requests for increased spending for Israeli national security, one major consequence being that the periods just before an election witness growth in the government military outlays. This conjecture is strongly supported by the empirical results as reflected by the significant, positive parameter β_6. Similarly, given the prominence of the military-industrial-governmental partnership in Israel, falling profits (primarily in the three defense-oriented monopolies) are seen to motivate some increases in domestic military spending. This result accords with the conjecture that even in highly security-conscious societies like Israel, the government is able to use military spending at the margins to influence the prevailing macroeconomic conditions. Quite interesting, also, is the fact that the adjustment speed of Israeli domestic spending is very slow, suggesting that the role of accumulated history is quite significant. Hence, it appears that for Israel, at least, the importance of prior security threats as well as of prior domestic political-economic circumstances weighs very heavily (i.e., are not heavily discounted) in calculating current defense needs.

The least accurate representation is of U.S. military aid to Israel, whose abrupt fluctuations are not well captured by the model. As seen in Figure 3, the overall fit of the model to the military aid series is adequate, but the major fluctuations—primarily caused by war periods—are not adequately reflected in the estimated relationships. Again, security concerns are the most important here, as seen in the positive, significant parameter for the weight of Arab military spending in establishing goals for U.S. military aid. The intensification programs of the Israeli Defense Forces were not found to play an important role in these aid targets, as shown by the negligible value for β_9, probably because of the contemporaneous impact of the Arab postwar intensification programs. On the political-economic side, the empirical results again point to the presence of a high degree of sensitivity to the political process not only in Israel but also in the United States. Thus, the impact of U.S. presidential election cycles (β_{11}) is to drive up the granting of U.S. military aid to Israel. In a manner similar to the pattern found for domestic military spending, the adjustment of actual U.S. aid to Israel to its desired or negotiated level is fairly slow and the impact of the past is not very heavily discounted. In addition to the military aid, it is important to note that Israel also receives economic aid of $785 million per year from the United States. This aid is officially earmarked for civilian projects but relieves some of the pressure on the national budget and hence indirectly helps to finance military programs (McGuire 1982).

Figure 4 portrays the total amount of Israeli foreign military imports. The overall fit of the model to the data is again strong, although, in contrast to the first two components of the model, the adjustment speed of Israeli foreign military imports to the level of U.S. military aid to Israel is virtually instantaneous—1.53. This means that Israel makes its total military imports almost identical to the amount of aid it receives from the United States. Finally, Figure 5 illustrates the congruence of the total Israeli military

Figure 5. Total Israeli Defense Expenditures (1975 Shekels)



budget, with the total constructed from the estimated components, a further check on the overall coherence of the model. The visual fit of both the level and the major turning points is quite suggestive of the high correspondence between model predictions and actual outcomes.

All in all, the ideas set forth have been remarkably robust in suggesting the broad outline of the military-security, as well as political-economic, influences on the evolution of the military budget in one industrializing nation with a growing domestic defense industry—Israel. The derived model and the actual data are remarkably similar, and the estimated parameters are significant and interpretable in terms of both the model and the history of Israeli defense politics.

Conclusion

Our message is simple. Even in highly security-conscious societies such as Israel, the government uses the defense budget at the margins to respond to political and economic pressures. While security considerations play the key role in influencing military spending, electoral competition and concerns for the economic well-being of major corporations also influence spending. The military in Israel is the government's single most important fiscal mechanism for stimulating the economy and influencing elections, and the government is simply not able to overlook its political and economic significance.

The importance of domestic political-economic factors in influencing defense expenditures in the United States, the Soviet Union, and many Western European countries is well documented. Since these political-economic influences may be seen in the Israeli case, they appear to be more general than previously shown. Israel presents a "hard test" for this thesis. In particular, these influences should not be overlooked in the analysis of military expenditures of less-developed and newly developed societies. If a society like Israel, which faces continuing and periodically intensified threats to its security, also utilizes the defense budget (albeit at the margins) as an instrument of macroeconomic and electoral policy, such practices would appear to be widespread. Accordingly, in societies with lower levels of conflict involvement than Israel, the defense budget is likely to be used more widely for nonmilitary purposes, especially if these societies also possess a significant local armament industry. Although current knowledge is insufficient for sweeping claims of generality, empirical and theoretical evidence converges in suggesting the prevalence of military as well as nonmilitary uses of defense budgets in many countries. The emergence of indigenous defense industries in such heterogeneous societies as Argentina, Brazil, India, Egypt, South Korea, Pakistan, and Taiwan, makes the defense budget a very likely candidate for political and economic manipulation. The implications of this study go beyond the Israeli case and point to the potential importance of viewing the defense budget as one of the primary political-economic tools of national decision makers.

Notes

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1. We recognize that it is controversial to define Israel along developmental dimensions or to place it in the Third World. Peri (1983, 3) explained, "From the point of view of its geographical location, its period of political independence and certain aspects of its social fabric, Israel appeared at one end of the spectrum, while its economic characteristics, political regime and cultural system seemed to point to the other extreme." Nonetheless, virtually all studies of *arms production* treat Israel as a developing Third World nation (e.g., Brzoska and Ohlson 1986; Katz 1984; and Neuman 1984).

2. *Monthly Bureau of Statistics*, supplements to vols. 4 (1982) and 36 (1985); and *Statistical Abstract of Israel*.

3. *International Financial Statistics*, with the *Supplement on Exchange Rates* no. 9 (1985) and *Supplement on Price Statistics* no. 12 (1986).

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COMMUNITY, INTEGRATION, AND STABILITY IN MULTINATIONAL YUGOSLAVIA

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In 1981 a higher proportion of the multinational population of Yugoslavia chose to declare "Yugoslav" in place of an ethnic identity in response to the census question on nationality than ever before. We present arguments to support the interpretation of Yugoslav identity as evidence of shared political identity, and carry out an analysis of aggregate data on the level of social and material development, political socialization, and interethnic contact in the country's nearly five hundred counties to discover the sources of that identity. We find that with certain important regional variations, Yugoslav identity seems to be the product of interethnic contact and higher education rather than the level of material well-being. These findings support an interpretation of Yugoslav identity as evidence of diffuse support for the existence of a shared political community and suggest both the sources and vulnerabilities of Yugoslav stability.

The stability of the communist political order in Yugoslavia rests in large part on the party's ability to maintain social and political peace among the nationalities. Internationality conflict and nationalist unrest have challenged that stability in the past and constitute potential bases for such challenges in the future. Yet between the 1971 and 1981 censuses in Yugoslavia, the number of individuals who declared Yugoslav identity increased 4½-fold, from 273,077 to 1,219,024 persons, or from 1.3% to 5.4% of the population. The country is divided into almost five hundred counties with a median size of 446 square kilometers and a median population of 29,329. In many of those counties more than 10% of the population declared Yugoslav identity. In response to a more recent survey conducted by Yugoslav sociologists (Flere 1987), some 15% of Yugoslav youth declared Yugoslav identity and some 36% expressed a "preference" for declaring it in place of their ethnic identity, suggesting that the tendency to declare Yugoslav identity has continued to spread.

The increase in the number of "Yugoslavs" in the 1981 census far exceeded the expected natural increment of this group and can be attributed almost entirely to the declaration of this identity by individuals who had declared another ethnic identity in 1971 (see Appendix A). In some areas "Yugoslavs" increased from near 0 in 1971 to over 10% in 1981. In other areas the proportion of "Yugoslavs" has increased to over 15% of the population. In a few places it is now as much as 25%. Given the ethnic, social, demographic, and economic diversity of these counties, the declaration of Yugoslav identity by such proportions of their populations constituted a remarkable assertion of shared political identity, or "sense of community." It is a clear indication of increased political integration and, given the particular meaning of such a declaration in the Yugoslav political setting, also implied increased levels of diffuse support for the regime.

We present arguments to support the interpretation of Yugoslav identity as an indicator of political integration and diffuse regime support. We attempt, in turn, to explain the level of Yugoslav identification in terms of four, broad, social processes often associated with social mobilization and political integration: (1) material development, (2) the delivery of social services, (3) political socialization, and (4) intergroup contact. By doing so, we hope to be able to discover the nature and sources of political stability in Yugoslavia.

Community, Integration, and Stability in Multinational States

Cultural diversity, or multinationality, is widely viewed in the comparative politics literature as an impediment to integration at the mass level and therefore as a threat to political stability. In studies of single states the level of political integration at the mass level is often defined in terms suggested by Karl Deutsch, that is, in terms of the extent to which "a sense of community" has been achieved in the population (1953, 1957). The emergence of such a sense of community among the population of a multinational state need not necessarily be accompanied by the assimilation of cultural or national minorities, or the "homogenization" of society. As Amitai Etzioni suggests, even in an integrated political community "identification with the community is necessary only in political matters. Identification in other areas . . . might be less or more encompassing" (1965, 5). In effect, the process of integration in a multinational state can be viewed as the emergence of what Morris Janowitz calls "civic consciousness," or "positive and meaningful attachments" to the political community. It is, in Janowitz's terms, "the process by which national attachments and obligations are molded into the search for supranational citizenship" (Janowitz 1983, x–xi).

Some scholars argue that political integration in the multinational state is enhanced by interethnic contact. Deutsch's "communications theory" of national identity formation, for example, is based very largely on indicators of such contact (1953, 70–79). More recently, Arend Lijphart and other theorists of the "consociationalism" approach have contested this view. They argue that integration is enhanced by the isolation of ethnic communities at the mass level, with contact reserved for ethnic elites (Lijphart 1975, 1977; McRae 1974).

Empirical studies of integration have been handicapped by the difficulty of defining a reliable measure or indicator of the "sense of community" in the population. Deutsch's own work simply asserts that broad, socioeconomic developmental processes, subsumed under the rubric *social mobilization* may result in "integration" (1953, 1961). Essentially conceptual in its focus, it offers little empirical evidence to link these processes to changes in individual attitudes or behavior in the direction of an increasing "sense of community." Michael Hechter attempts to provide such evidence in his more recent study of the political economy of multiethnic integration in the United Kingdom (1975). Using county-level aggregate data of the type Deutsch suggests might be useful for the study of integration and nation building, Hechter explores the relationships among development (primarily industrialization), and persistence of ethnic (Celtic) identification, and support for the multinational regime, or political "community."

Most contemporary research on political integration and regime support, however, has been based on the analysis of survey-generated data. Gary Bertsch, for example, carried out a survey-based study of the "community-building process" in Yugoslavia in the late 1960s. He finds that social mobilization gave rise to a shift from traditional to modern values and, thereby, from particularistic to universalistic patterns of identification. "In a complex multinational state such as the Yugoslav," he argues, "mobilization, modernism, and universalism are likely to engender the behaviors on which communities are based" (1972, 444). More recently, Allan Kornberg and colleagues have used survey data to explore the sources of "support for community" and "regime support" in the regions of Canada as a means of assessing the capacity of the Canadian regime to maintain its "integrity" (Kornberg, Clarke, and LeDuc 1978; Kornberg, Clarke, and Stewart 1980). They find that the level of political partisanship and intensity of party identification—factors that one might also interpret as indicators of mobilization—were powerful predictors of the level of support for community in Canada.

Evidence of the behavioral consequences of varying levels of support has been provided in recent studies by Muller and his colleagues. They establish the existence of an inverse relationship between regime support on the one hand and anti-regime or "aggressive" political behavior on the other (Muller 1979; Muller and Jukam 1977; Muller, Jukam, and Seligson 1982; Muller and Williams 1980). Muller's research generates important empirical evidence that reinforces David Easton's earlier distinction between *specific* and *diffuse* support. Easton argues that specific support arises out of "the satisfactions that members of a system feel they obtain from the perceived outputs and performance of the political authorities." But, he continues, "it is only indirectly relevant, if at all, to the input of support for the regime or political community." Diffuse support, on the other hand, represents a "reservoir of favorable attitudes or good will that helps members to accept or tolerate outputs to which they are opposed or the effects of which they see as damaging to their wants." It is produced, Easton argues, by "childhood and continuing adult socialization" and may be manifest "as a sense of we-feeling, common consciousness, or group identification" (1975, 437, 444–45). Muller demonstrates that the level of diffuse support is far more powerfully related to aggressive political behavior than is the level of specific support. And this suggests that the level of diffuse support, manifest in the "sense of community," is most relevant to the study of political integration—and ultimately political stability—in multinational regimes.

Measuring "Community" and "Support"

Reliance on survey-generated data to develop an indicator of "community" in Yugoslavia is problematic. Indigenous scholars have only recently returned to large-scale survey research, and permission for foreign scholars to conduct such research may still be years away. Moreover, such research has until now proven unable to determine the causal direction of the relationship between objective social and economic conditions and therefore also of regime policy "outputs" on the one hand and the level of regime support in the population on the other (Muller and Williams 1980).

Developing an acceptable indicator of political community has also proven problematic. Kornberg relies on a direct question, using a "thermometer scale" technique to record the level of support. Muller developed a set of attitudinal questions that, when scaled, seemed effectively to measure "system affect," or political support for the regime. Research based on aggregate data focuses on electoral behavior for this purpose. (Hechter, for example, uses aggregate electoral support for the British Conservative party as his indicator). But the utility of doing so in a single-party state with a noncompetitive electoral system appears limited. Cohen and Warwick (1983), for example, attempt to circumvent this problem by using nonvoting in Yugoslav elections.[1]

Data that can be used as an alternative indicator of the "sense of community" or level of "diffuse support" for the existing political community is, however, readily available in the Yugoslav case. It is census data on the declaration of Yugoslav identity in response to the open-ended question, "What is your nationality?"

"Yugoslav" Identity and Political Community

"Yugoslav" is not an easy answer to give in response to the census question on nationality. After a brief attempt to encourage "Yugoslavism" in the late 1950s, the official policy of the regime on this issue quickly became, if not anti-"Yugoslav," at the very least pronational. In 1958 the central leadership had advanced a formal theory of nationalism and adopted a party program, both of which called for the emergence of a supranational, Yugoslav identity. But with the devolution of increasing power to the leaderships of the ethnically based republics and provinces in the early 1960s, this attempt to encourage "Yugoslavism" was abandoned (Shoup 1968, 201–11).

Since 1966 Yugoslav leaders have pursued a "consociational" strategy for controlling ethnic conflict. They have deliberately fostered segmentation of the social, economic, and political systems along ethnic and regional cleavages, reserving cross-cleavage interaction for ruling party elites (Burg 1983a). Since then, regional leaderships have seen any effort to encourage Yugoslavism as an attempt to undermine their respective power bases and to encourage support for a more centralized political order. Consequently, they have opposed any official attempt to foster the emergence of an identity that might shift the focus of popular loyalties from the regions to the federal center.

This reluctance formally to encourage the emergence of Yugoslav identity was reflected in the official instructions to census-takers concerning answers to the nationality question. In 1981 they stated that "if a citizen wishes to record Yugoslav as an answer to this question, the census taker is obliged to record *even* that answer, although by this the citizen does not declare membership in a nation or nationality" (Savezni Zavod za Statistiku 1981, 38; emphasis added).

Over time the meaning of Yugoslav identity appears to have changed. Until 1961 the category was used by the traditionally Islamic but ethnically Slavic population—for whom no category had yet been provided—to avoid having to declare either Serbian or Croatian identity. For the 1961 census the category "Muslim in the ethnic sense" was introduced. Nonetheless, significant numbers of Muslims appear to have continued to declare Yugoslav identity. Only in 1971, with the introduction of the category "Muslim in the national sense," was the response "Yugoslav" divorced from its past association with the Muslims (Burg 1983b).

The growing sensitivity of the national question in the late 1960s led to explicit discussions of the meaning of Yugoslavism. As the 1971 census drew closer, the meaning of declaring Yugoslav identity became the subject of polemical debates. These appear to have resulted in the equation of Yugoslavism with socialist patriotism and the classification of Yugoslav as a political rather than ethnic identity (Bakic 1985). Moreover, at the time the 1971 census was being taken, internationality conflict resulting from the rise of assertive nationalisms had already reached crisis proportions (Burg 1983a; Rusinow 1977).

The most serious challenge to the legitimacy of the multinational community was posed by Croatian student leaders and cultural elites who had mobilized nationalist sentiments and organized a popular, alternative political movement in that republic. Mobilized nationalisms in other regions also threatened to destroy the political community, but none represented as serious a threat as Croatian nationalism. Centered in the republic's universities, cultural institutions, and mass media, the Croatian nationalist movement quickly gained mass support. Its leaders advanced increasingly radical demands, and even came to challenge the communist party leadership for control over the republic.

In late 1971 and 1972 this and the other regional political movements challenging the existing political order were forcibly suppressed, and their leadership arrested, purged, imprisoned, or forced out of public life. But their impact on Yugoslav political development was both powerful and long-lasting: they strengthened an already-existing tendency toward extensive devolution of political power and authority to the republics and provinces at the expense of the federation, a tendency that remains a powerful force in Yugoslav politics even today (Burg 1986).

Those who declared "Yugoslav" rather than an ethnic identity in 1971 were deciding in favor of an option that clearly was contrary to the overwhelmingly centrifugal tendencies of the moment. At the very least, the declaration of Yugoslav identity in 1971 can be interpreted as an indication of the conscious rejection of a fissiparous, or exclusivist, orientation in favor of a more integrative, or universalist, one. By 1981, however, the definition of Yugoslav as a political identity appears to have become more clearly established in both the popular press and more scholarly works (Dugandzija 1984; Matvejevic 1983). From this perspective, Yugoslav identity can be interpreted as an expression of what Janowitz has called "civic consciousness" or what one Yugoslav political scientist has called a "transnational" sense of "belonging to the society, the community" (Miric 1984, 109).

Some behavioral evidence of an association between "Yugoslav" identity and support for the overarching political community can be found in the overrepresentation of "Yugoslavs" in the Party. (All party data are from Cvjeticanin 1984.) While only 1.3% of the population in 1971, "Yugoslavs" constituted 3.9% of the Party. In 1981 they constituted 6.5% of the Party (7.1% in 1982). The proportion of "Yugoslavs" in each of the regional party organizations for which they were reported increased steadily from 1972 to 1982, despite the overall growth of these parties. By 1982, they were overrepresented in the party organizations of Slovenia, Croatia, Bosnia, and Serbia. More importantly, "Yugoslavs" were also overrepresented in those sectors of the party where we might expect commitment to the regime to be highest: the military and federal party organizations.

In 1981 "Yugoslavs" constituted 8.0% of the military party organization, which comprises primarily the officer corps. In 1982 they were 8.8%. In the federal party organization, which comprises all party members employed in federal institutions and organizations, "Yugoslavs" constituted 19.5% of the total in 1981 and 18.0% in 1982. While this suggests a strong association between Yugoslav identity and support for the shared political community, it is important to note that the actual strength of that relationship may be even greater. Regional leaderships exercise considerable influence over appointments to the federal apparatus. This represents a strong incentive to declare the titular national identity of one's home region, and may suppress the declaration of Yugoslav identity.

If we assume that membership in the party reflects support for the multinational political community, these data are consistent with the interpretation of

Yugoslav identity as evidence of such support. However, the declaration of Yugoslav identity does not necessarily indicate *specific* support. It may indicate only *diffuse* support for the idea of a shared, multinational, political community. As Easton has suggested, such support does not preclude opposition to the incumbent leadership and its policies.

An Alternative Hypothesis

The dramatic increase in the number of "Yugoslavs" in 1981 led to renewed denials of, and opposition to, any official effort to encourage the emergence of a Yugoslav *ethnic* identity. It also led some Yugoslav observers to suggest explanations for the increase having little to do with popular support for the regime.[2] Foremost among these was the attempt to attribute the declaration of Yugoslav identity to the effect, on both the marriage partners and their offspring, of mixed marriages.

Were county-level data on the rate of mixed marriages to become available, it would hardly be surprising to find a strong correlation between such rates and the declaration of Yugoslav identity. The selection of an ethnically distinct marriage partner has long been recognized in studies of ethnocentrism and ethnic distance as the strongest evidence of ethnic tolerance or openness, surely a precondition to the emergence of a "sense of community" in a multiethnic setting.

In the absence of cross-time data on the marital status and nationality declarations of individuals, however, it is impossible to determine directly whether intermarriage results in a shift from ethnic to Yugoslav identity. However, the effect of ethnic intermarriage on the declaration of Yugoslav identity by offspring can be subjected to such a test. Since parents report the national identity of their children under 15 years of age, we used census data on age to create a "generation" of individuals who attained the age of 15 in the intercensal period and were declaring their identity on their own for the first time in 1981. This generation included the offspring of mixed marriages concluded in the 1960s. If the increase in the number of "Yugoslavs" reflected an increase in the number of individuals who were the offspring of mixed marriages, we would expect strong positive correlations between the proportion of the population in this generation and the proportion that declared Yugoslav identity. Yet, as will be shown, no such relationship could be established.

A more refined interpretation of the relationship between mixed marriages and Yugoslav identity was offered by the results of a recent survey of attitudes and values among youths, conducted by a group of Yugoslav sociologists. These researchers asked their respondents to distinguish between their "objective" nationality and their "preferred" national identification. While 34% of the offspring of mixed marriages declared Yugoslav identity, approximately 14% of the children of homogeneous marriages also did so. And whereas 51% of the offspring of mixed marriages "preferred" to identify as "Yugoslav," approximately 35% of the children of homogeneous marriages also "preferred" to do so (Flere 1987; calculations based on his Table 2, p. 11).

A 1982 survey of youths from all over Yugoslavia participating in political summer camps in Croatia yielded similar results: 38.4% of the 1,729 respondents declared Yugoslav identity, and 51% reported they "felt themselves" to be "just Yugoslav." While 52.6% of the offspring of mixed marriages declared Yugoslav identity, 39.4% of the children of homogenous marriages also did so. Moreover, the children of mixed marriages comprised only 16.3% of these politically mobilized youth, clearly not a large-enough group to account for the high proportion of "Yugoslavs" (Obradovic 1984, 140–44). These findings suggest that mixed marriages may be intensifying processes that are taking place more broadly in Yugoslav society, or even contributing to re-identification independently. But it is very unlikely that mixed marriages alone account for the scope of Yugoslav identification. It is far more likely that both Yugoslav identity and mixed marriages are independently associated with ethnic interaction (cf. Obradovic 1984, 250).

The Correlates of Yugoslav Identity

The Research Design

Using the proportion of "Yugoslavs" in the population of each county as an indicator of support for a shared political community and as the dependent variable in our analyses, we attempt to explain the level of support in terms of four broad social processes: (1) material development, as measured by per capita income (which is very strongly and positively correlated with such other indicators as per capita gross material production and industrial employment and negatively correlated with such indicators of underdevelopment as the proportion of peasants in the population) and by the availability of expendable income (i.e., the extent of television ownership) in each county; (2) the delivery of social services, as measured by the number of doctors and dentists per thousand population (which is highly correlated with the number of individuals employed in education, culture, health, and other social services and the number of hospital beds per thousand population); (3) political socialization, as measured by the proportion of the population in each county with higher education and by newspaper circulation (television ownership is relevant here, as well); (4) intergroup contact, as measured by both the degree of ethnic heterogeneity, or "fragmentation," of the county population;[3] and (for 1981) the proportion of the population that had already declared Yugoslav identity in 1971.

In addition, we explored the effect of generational differences in the population on the extent of Yugoslav identification. We did this in terms of "political generations," defined as age cohorts whose adolescent and young adult years came during (1) *World War II* and the revolution, (2) the postwar *de-Stalinization*, (3) the political *liberalization* of the late 50s and early 60s, (4) the rapid *regionalization* and the rise of ethnic nationalisms in the late sixties and early seventies, and (5) the *consolidation* of the decentralized system in the later 1970s.

The material and social development variables were intended to measure two dimensions of policy "outputs." If Yugoslav identity is primarily a manifestation of specific support, these variables can be expected to be powerful predictors of it. If, on the other hand, Yugoslav identity is a manifestation of the level of diffuse support, we should expect the measures of political socialization, intergroup contact, and generational experiences to be more powerful predictors of the proportion of the population declaring Yugoslav identity.

The proportion of "Yugoslavs" in any county is the product of both the personal characteristics of individuals, such as life history and personality, and the social and economic characteristics of both individuals and the county setting itself. No personalistic variables were available in our census data and in any event the distribution of variations in these characteristics should not vary much across county populations. Our analysis was conducted entirely on the basis of aggregate data about the social and economic characteristics of county populations. Our intention was to relate the occurrence of a key political behavior—the declaration of Yugoslav identity—to fundamental variations in the characteristics of these social collectivities.

Aggregation biases can arise through

Table 1. Median County Values on Predictor Variables, 1981

		Regions							
		South					North		
Variables	All Yugoslavia	Kosovo	Bosnia	Serbia	Montenegro	Macedonia	Vojvodina	Croatia	Slovenia
Material development									
Per capita income (dinars)	15,928	4,697	12,241	12,429	13,573	13,880	17,015	20,146	34,310
TVs per thousand	166.8	40.0	118.7	143.1	115.6	118.1	219.3	208.7	255.1
Social services									
Doctors per thousand	.9	.4	.7	1.0	1.0	.9	1.1	1.1	1.0
Socialization									
% with higher education	2.8	1.9	2.6	2.4	4.0	1.9	3.2	3.3	3.6
Newspaper circulation per thousand	347.1	.9	164.2	149.8	532.1	296.7	499.3	592.0	2433.0
Contact									
Fragmentation	.28	.29	.52	.11	.40	.22	.53	.33	.11
% "Yugoslav" 1971	.42	.03	.40	.33	.65	.06	1.32	1.05	.20
Generations (%)									
War	18.8	8.7	12.0	21.6	16.3	14.0	22.2	22.7	18.9
De-Stalinization	13.3	7.2	12.1	16.1	11.2	10.7	15.1	14.3	12.6
Liberalization	11.9	9.1	11.5	12.0	11.8	11.6	12.6	11.8	12.1
Regionalization	15.2	12.4	15.1	15.4	14.7	15.9	15.3	14.5	16.0
Consolidation	16.1	19.8	21.1	14.0	18.7	18.9	14.2	15.0	16.0
Under age 15	22.2	42.0	26.3	20.0	25.2	26.9	19.2	20.1	22.8
Number of counties	486	22	104	107	20	30	44	103	56

omission from the model of important explanatory variables. Given the broad inclusion of aggregate-level variables from each of four theoretical viewpoints in our model, omitted variables in the present analyses were most likely to be of the personalistic kind, that is, variables that distinguished individuals. These may reliably affect declaration of Yugoslav identity. But the bias introduced by their omission will be small if the basis of grouping was such that little or no correlation across the groups (counties) existed between these omitted personalistic variables and the other aggregate variables that were included (Hanushek, Jackson, and Kain 1974). Most of the personalistic variables that might affect the declaration of ethnic identity must be distributed randomly across the 486 counties, so the correlations that could produce bias was presumably near zero in magnitude. Of course, we cannot support a claim that this is true for all such variables; possibility of some aggregation bias in our coefficient estimates cannot be denied. On the whole, however, we consider that we were much closer to the best than to the worst case[4] and further believe that our conclusions would be little altered by elimination of any remaining aggregation biases from our estimates. The consistently high R-squareds associated with our models support this conclusion.

As the data displayed in Table 1 make clear, the social and economic characteristics of counties and their populations vary enormously across the regions, so that analysis at the all-Yugoslavia level alone is likely to obscure important regional variations in the statistical relationships among our variables. The country as a whole is divided between historically, culturally, and developmentally distinct northern and southern zones (Burks 1971). The republics of Slovenia and Croatia and the autonomous province of Vojvodina (a part of the republic of Serbia) constitute the relatively highly developed north, whereas the republics of Bosnia and Hercegovina, Montenegro, and Macedonia, and the autonomous province of Kosovo (another part of Serbia) constitute the underdeveloped south. "Serbia proper," that is, Serbian territory apart from the autonomous provinces, can also be considered part of the less developed south. The magnitude of developmental differences between the north and south is reflected in the data presented in Table 1. The median per capita income in the counties of the most-developed northern region, Slovenia, for example, is six times that in the least-developed southern region, Kosovo. The median county values of almost all our other predictor variables are also characterized by similarly sharp regional differences. Even within the north and the south, however, the individual republics and provinces are characterized by distinct political histories and cultural development and by important differences in ethnonational composition and demographic trends.

Such sharp regional differences must be taken into account in any analysis. We approached them in much the same way as Kornberg and his collaborators did in their study of public support for "community" and "regime" in the regions of Canada (Kornberg, Clarke, and Stewart 1980); that is, we treated them first as regionally distributed variations within a single pattern or set of processes by introducing a set of dummy variables for region into an all-Yugoslavia analysis. Second, we treated them as reflections of the fact that the republics and provinces are distinct places characterized by distinct processes. We therefore carried out eight separate but, for purposes of comparison, parallel analyses.[5] The differing results produced by these two approaches are illustrated by the simple correlations between our predictor and dependent variables, displayed in Table 2. Note, for example, that the correlation between per capita income and the proportion of

Table 2. Pearson Correlations between Explanatory Variables and Proportion of "Yugoslavs" in the Population, 1981

		Regions							
		South					North		
Variables	All Yugoslavia	Kosovo	Bosnia	Serbia	Montenegro	Macedonia	Vojvodina	Croatia	Slovenia
Material development									
Per capita income (dinars)	.25	.56	.65	.38	.55	.29	.36	.41	.52
TVs per thousand	.31	.57	.72	.12	.74	.08	.29	.36	.29
Social services									
Doctors per thousand	.36	.56	.57	.35	.77	.36	.19	.33	.44
Socialization									
% with higher education	.48	.66	.69	.47	.80	.23	.43	.38	.67
Newspaper circulation per thousand	.12	.28	.54	.26	-.02	.47	.30	.25	.24
Contact									
Fragmentation	.59	.53	.50	.55	.70	.45	.38	.80	.80
% "Yugoslav" 1971	.80	.75	.82	.56	.99	.55	.81	.82	.77
Generations (%)									
War	.06	.32	-.05	-.02	.19	.23	-.34	-.10	-.15
De-Stalinization	.20	.47	.37	-.09	.41	-.03	.29	.20	.23
Liberalization	.27	.27	.59	.20	.55	.03	.22	.12	.24
Regionalization	.28	.54	.59	.20	.73	.17	.31	.36	.69
Consolidation	-.12	.09	-.42	-.07	-.74	-.14	.17	-.10	-.42
Number of cases	486	22	104	107	20	30	44	103	56

"Yugoslavs" was only .25 for the country as a whole, while it varied from .29 to .65 in the regions.

The relationships in each region between our predictor and dependent variables are likely to differ in character, as well as in magnitude. The relationships between higher education and "Yugoslav" identity in Croatia, for example, is almost certain to be very different from that in the other regions. Both the magnitude and the character of these relationships can be expected to differ over time as well. Devolution of power and authority to the regions and their increasing control over public policies, for example, are likely to dilute the impact of policy outcomes on the degree of support for the overarching multinational political community. Thus, any attempt to explain the sources of Yugoslav identity in 1981 must take into account both interregional and cross-time differences in the relationships between variables.

Findings

Although it is impossible to measure the unique contributions of each explanatory variable to the declaration of Yugoslav identity, we developed an approximation of their relative importance by applying multiple linear regression analysis techniques to our data.[6] For purposes of "intelligibility," we present the results of our regression analyses in the form of standardized regression coefficients (Converse and Pierce 1986, 958, n. 7). These are displayed in Table 3. Since our cases comprise *all* existing counties, rather then a sample of them, these should be treated as *descriptive* measures rather than inferential ones.

Analysis at the all-Yugoslavia level produced strong results, accounting for about 72% of the variation in the proportion of "Yugoslavs" among the 486 counties. With all of the explanatory variables taken into account simultaneously, the proportion of "Yugoslavs" in 1971 and fragmentation (our measure of the level of interethnic contact) were both strongly and positively associated with the declaration of Yugoslav identity in 1981. The proportions of the population in the *de-Stalinization* generation, and with higher education were moderately associated with the declaration of Yugoslav identity. The extent of newspaper readership was moderately and *negatively* associated with "Yugoslavs." No other variable approached these in relative importance.

Contrary to the hypothesized effect of interethnic marriage on the identification of offspring, there was essentially no association between the proportion of the population in the "consolidation" generation (comprising individuals declaring their identity on their own for the first time) and the proportion of "Yugoslavs" in the population. Similarly, neither per capita income in 1980 nor the extent of television ownership—both good indicators of material well-being—were strongly associated with regime support as manifested in the declaration of Yugoslav identity, contrary to expectations.

By adding a set of dummy variables for region to this analysis (the all-Yugoslavia-plus-region column and the dummy-coefficient-for-all-Yugoslavia-plus-region row), we produced even stronger results. The proportion of variation in "Yugoslavs" accounted for by this model increased to about 77%. The addition of region also resulted in a sharp decline in the relative strengths of education and newspaper circulation as predictors of Yugoslav identity. The strength of the association between interethnic contact and "Yugoslavs," in contrast, remained essentially unchanged. And the strength of the proportion of "Yugoslavs" in 1971 as a predictor of "Yugoslavs" in 1981 actually increased.

The regression coefficients for the dummy variables for region suggest that even if we assume that the socioeconomic proc-

Table 3. Standardized Regression Coefficients for 1980–81

			Regions							
			South					North		
Variables	All Yugoslavia	All Yugoslavia + Region	Kosovo	Bosnia	Serbia	Montenegro	Macedonia	Vojvodina	Croatia	Slovenia
Material development										
Per capita income (dinars)	-.041	.027	.234	.165	-.243	.171	.452	-.204	.039	-.161
TVs per thousand	.060	.071	.243	.135	-1.413	.126	-.066	.291	.054	-.040
Social services										
Doctors per thousand	-.035	-.034	-.255	-.053	.364	-.200	.612	-.260	.070	.018
Socialization										
% with higher education	.114	.017	.676	-.272	2.039	-.137	-.020	.222	-.265	.564
Newspaper circulation per thousand	-.103	-.076	-.417	.119	-1.184	.026	—	-.009	-.036	-.227
Contact										
Fragmentation	.325	.297	.065	.153	-.127	.019	.350	-.034	.538	.143
% "Yugoslav" 1971	.599	.615	.568	.558	.254	1.154	.101	.805	.474	.462
Generations (%)										
War	.083	.054	-.169	-.094	.331	-.000	.187	.047	.002	.086
De-Stalinization	.142	.082	.752	.192	-.070	.077	-.200	.251	.025	.146
Liberalization	-.006	.005	-.491	-.141	-.024	-.119	-.679	-.013	-.061	-.009
Regionalization	.000	.020	-.285	.125	.083	.020	.242	.127	.165	.247
Consolidation	.070	-.093	-.064	-.411	-.369	.089	-.147	-.087	-.084	.131
R^2	.73	.78	.92	.83	.61	1.00	.64	.77	.83	.84
Adjusted R^2	.72	.77	.81	.80	.57	.99	.40	.68	.81	.80
Number of cases	486	486	22	104	107	20	30	44	103	56
Dummy coefficient for all Yugoslavia + region	—	—	-.028	.075	-.120	-.073	-.095	-.186	—[a]	-.166

[a] Base category for region.

esses that produce "Yugoslavs" are the same everywhere, these processes will produce marginally smaller proportions of "Yugoslavs" in Montenegro, Macedonia, Slovenia, Serbia, Kosovo, and Vojvodina than in Croatia and will produce a marginally greater proportion of "Yugoslavs" in Bosnia. We explored these regional effects more closely by subjecting the data in each republic and province to separate but identical analyses. The results of identical regional regressions are displayed in the regions columns in Table 3. The most striking results of this approach were, first, that the pattern of associations varied dramatically across the regions, a result consistent with the vast economic, cultural, and other differences between them, noted above; and, second, that with the exceptions of Macedonia and Serbia, regional-level analysis offered even greater explanatory power. Note that in five of the eight regions we again accounted for more than three-quarters of the variation in our dependent variable.

Our findings with respect to material development reflect the north-south division in Yugoslavia. Our indicator of material development—per capita income—was relatively moderately and positively associated with Yugoslav identity in Bosnia, Montenegro, Macedonia, and Kosovo. These four regions constitute most of the underdeveloped south of the country and are heavily dependent on the central government for developmental assistance. It is not surprising, therefore, that higher levels of development in these regions should be associated with higher levels of political support for the multinational community and, therefore, higher proportions of "Yugoslavs." Our indicator of social development—the number of doctors per thousand population—yielded mixed results in these regions, a finding consistent with the narrower interregional differences on this dimension reported in Table 1.

The association between the proportion of "Yugoslavs" in 1971 and the proportion in 1981 was positive and relatively strong in every region except Macedonia and Serbia, where it was positive but relatively weak. The association between interethnic contact and Yugoslav identity was relatively strong and positive only in Croatia and Macedonia. In Croatia the level of interethnic contact was, in fact, the strongest predictor of Yugoslav identity. Thus, except in Serbia, either the level of interethnic contact or the proportion of the population that had already declared Yugoslav identity in 1971 or both were among the strongest predictors of Yugoslav identity in 1981. These findings support the proposition that interethnic contact contributes to political integration.

The strong positive association between the proportion of "Yugoslavs" in 1971 and the proportion in 1981 suggests that Yugoslav identity may be spread through a process of socialization. Indeed, consistent with this view, the association between the proportion of the population with higher education and the proportion of "Yugoslavs" was strongly positive in Serbia, Kosovo, and Slovenia. But it was strongly *negative* in Croatia—where nationalism has been strongest in the universities—and in Bosnia. Newspaper circulation, our other indicator of exposure to formal socialization processes, was positively associated with Yugoslav identity only in Bosnia, where the press tends to be more supportive of the central regime.

Interregional differences in the nature of socialization processes and their relationship to Yugoslav identity were reflected in the relationships between the proportions of the population in each of the five "political generations" and the extent of Yugoslav identity. No single generation was consistently and strongly associated with Yugoslav identity across all regions. Nor was there a consistent pattern of increasing or decreasing association across the generations.

Surprisingly, in both Slovenia and Croatia the proportion of the population in the *regionalization* generation, which came to political maturity in a period characterized by the rapid decentralization of power and authority to the regions and by the rise of nationalism in both these republics, was *positively* and, in comparison to other explanatory variables, moderately associated with the declaration of "Yugoslav" identity.

Contrary to the hypothesis that mixed marriages concluded in earlier years might have resulted in an increased rate of declaration of "Yugoslav" identity among those declaring their nationality on their own for the first time, the *consolidation* generation was not consistently more strongly associated with Yugoslav identity than the older generations. In most regions the relationship of the *consolidation* generation to Yugoslav identity was *negative* and relatively strongly so in Bosnia. Also contrary to expectations, the oldest, *war* generation was more strongly associated with Yugoslav identity than younger generations only in Serbia.

Overall, these findings suggest that the sources of Yugoslav identity and, by implication, the nature of support for the multinational community vary in important ways from region to region. Yugoslav identity is in part the product of material development, or policy "outputs," in the country's underdeveloped south. In these regions, therefore, support for community is at least partially specific in nature. But in almost every region, north and south, Yugoslav identity is the product of certain forms of socializing influences: interethnic contact, contact with already-declared "Yugoslavs," the completion of a higher education, or even formative political experiences. This indicates that the declaration of Yugoslav identity in 1981 was everywhere at least in part a reflection of diffuse support.

Cross-time Comparisons

In order to establish the cross-time validity of this analysis, we compared these results to results derived from an analysis of 1971 data identical to our 1981 data, with two important exceptions: first, newspaper circulation statistics were not available for 1970; second, there is no comparable measure of the number of "Yugoslavs" in the previous (1961) census.[7] Results of our analysis of 1971 data paralleled our 1981 findings.[8] Analysis at the all-Yugoslavia level indicated that in the period before the assertion of Yugoslav political identity by significant proportions of the population, the level of interethnic contact was the most powerful predictor of such identity. The proportion of the population with higher education was the next-most-powerful predictor.

At the regional level the pattern of associations in our 1971 data between social and material conditions and the declaration of Yugoslav identity was similar to that in 1981; that is, social and material development were relatively unimportant in the developed north but more important in the less-developed south. The relationship between interethnic contact and Yugoslav identity also paralleled the 1981 findings; that is, contact was important in every region except Serbia.

An important difference that emerged between our 1971 and 1981 results was the relationship between the proportions of the population with higher education and the proportion of "Yugoslavs." In the 1971 data, that relationship was moderately-to-strongly positive in all of the underdeveloped regions and in Vojvodina. It was moderately negative only in Slovenia, where a period of nationalist discontent had only recently come to an end. In Croatia, where nationalism was reaching its zenith, higher education and Yugoslav identity seemed unrelated in 1971. In our

1981 data, however, this relationship was negative in three of the five underdeveloped regions and strongly negative in Croatia.

These differences imply that the regionalization of the educational system that resulted from the devolution of the 1970s had eroded the socializing and integrative political effect of higher education in Yugoslavia. Indeed, our findings lend indirect support to the commonly held notion that a university education socializes one to Croatian nationalism in Croatia, tends to foster Bosnian Muslim ethnic and political identity in Bosnia, and contributes to the promulgation of Macedonian national identity in Macedonia. Only in Serbia has the completion of higher education remained strongly associated with the declaration of Yugoslav identity over time. And only in Slovenia has the relationship changed from negative to positive.

Similarly, the regionalization of control over the mass media during the 1970s helps to explain the absence of any consistent pattern of associations between Yugoslav identity and newspaper circulation in 1980.

Conclusions

Where diffuse support for a regime is sizable or increasing among the mass population, the political order can be expected to remain stable even during a period of declining regime performance. In the conflictual, multinational context of contemporary Yugoslavia, the declaration of Yugoslav identity can be seen as evidence of the "sense of community" associated with such diffuse support; and the increase between 1971 and 1981 in the proportion of the population that declared it suggests one reason for the stability of the political order until now in the face of sharply declining regime performance since 1981 (Burg 1986).

The unimportance of the level of material development as a predictor of popular support in the country's north and its relative weakness even in the country's south in the period 1971–81 may help to explain the surprising resilience of the regime in the face of the dramatic economic decline in the period since 1979. Our findings indicate that the diffuse support for the multinational community manifested in the declaration of Yugoslav identity was attributable in greater part to broad social processes that were largely independent of the level of development and can consequently be expected to continue to unfold for the foreseeable future: increasing ethnic interaction—especially the socializing influence of already-declared "Yugoslavs" on others—and, to a lesser extent, the spread of higher education.

The declaration of Yugoslav identity appears to fall into the category of what Schelling calls "critical mass phenomena" (1978, 91–102). In effect, the combination of higher education and interethnic contact induced individuals to declare Yugoslav identity in 1971, and the presence of these "Yugoslavs" increased the rate at which others decided to do the same. These findings support the view that while ethnic segmentation and isolation may be conducive to the establishment of elite cooperation and a "consociational" political order, interethnic contact and interaction is conducive to the emergence of the "sense of community" that fosters political integration at the mass level.

This implies that all other things being equal, the declaration of Yugoslav identity is likely to become more widespread, and the level of support for the multinational community is likely to increase. Some contemporary evidence suggests that this may in fact be the case. The postcensus surveys of Yugoslav youth cited earlier reported rates of declaration of Yugoslav identity many times greater than that of the 1981 census and astounding levels of "preference" or "feeling" for

Yugoslav identity (Flere 1987; Obradovic 1984).

But the key factor here would appear to be the ability of the regime to hold "all other things equal." We have already noted that our findings suggest a decline in the contribution of both higher education and exposure to the mass media to the level of diffuse support in Yugoslavia. It is also important to remember that the *absolute* levels of economic and social development of almost all the countries improved in the period 1971–81, their *relative* levels remaining largely unchanged. Fragmentary but mounting evidence indicates that the sharp decline in economic conditions since 1981 has had a substantial negative effect on popular assessments of the government and is resulting in increased levels of pessimism in the population.[9] Among intellectuals, students, and others, criticism of the regime's present incumbents and their policies and demands for democratization of the political order are increasing.[10]

Thus the regime is already experiencing an erosion of specific support. While short-term negative performance may not affect the level of diffuse support, sustained negative performance is likely to do so. If the Yugoslav leadership is to insulate diffuse support for the regime from such an erosion, it must first resolve the social, economic, and political problems that underlie the decline in economic performance and contribute to the erosion of specific support. But the findings reported here imply that in order to increase the level of diffuse support, the leadership must also revise its strategy for controlling ethnic conflict and devise solutions to break down ethnic isolation and permit increased levels of interethnic contact and to renew the formal socialization of regional populations to the Yugoslav idea. In the absence of such changes, it may be very difficult indeed to sustain diffuse support for a multinational political community in Yugoslavia.

Appendix A

Estimated 1981 Population Deficits and Surpluses, by Group

	A	B	C	D	E
Group	1971 Population	Projected Natural Increment 1971-80	1981 Population	1981 Deficit or Surplus (C minus [A + B])	D as % of (A + B)
Montenegrins	508,843	60,070	579,043	10,110	1.8
Croats	4,526,782	262,126	4,428,043	-360,865	-7.5
Macedonians	1,194,784	139,490	1,341,598	7,324	.5
Muslims	1,729,932	316,001	1,999,890	-49,043	-2.4
Slovenes	1,678,032	90,989	1,753,571	-15,450	-.9
Serbs	8,143,246	539,865	8,140,507	-537,126	-6.2
Albanians	1,309,523	441,780	1,730,878	-20,426	-1.2
Bulgarians	58,627	2,512	36,189	-24,950	-40.8
Hungarians	477,374	-7,568	426,867	-72,939	-15.5
Romanians	58,570	-1,354	54,955	-2,261	-4.0
Romi/Gypsies	78,485	25,289	168,197	64,423	62.1
Slovaks	83,656	1,209	80,334	-4,531	-5.3
Turks	127,920	16,787	101,291	-43,416	-30.0
"Yugoslavs"	273,077	55,227	1,219,024	890,720	271.3
Total	20,522,972	1,916,606	22,427,585	-11,933	-.1

Source: Ruza Petrovic, "The National Composition of the Population," *Yugoslav Survey* 24, no. 3 (August 1983), Tbl. 3, p. 26; *Statisticki Bilten,* no. 1295, p. 8.

Appendix B

Correlations of Explanatory Variables with 1981 Population Density

	Variables											
	Material Development		Social Services	Socialization		Contact		Generations (%)				
Regions	Per Capita Income	TVs per Thousand	Doctors per Thousand	% Higher Education	Newspaper Circulation per Thousand	Fragmentation	% "Yugoslav" 1971	War	De-Stalin	Liberalization	Regionalization	Consolidation
All Yugoslavia	.32	.28	.46	.60	.82	.25	.26	-.18	-.02	.28	.32	-.07
Kosovo	.37	.49	.46	.61	.51	.28	.35	-.63	-.43	.04	.18	-.15
Bosnia	.47	.53	.55	.65	.51	.28	.70	-.34	-.04	.39	.50	-.13
Serbia	.51	.56	.57	.78	.93	.51	.70	-.18	.00	.32	.35	-.08
Montenegro	.27	.66	.28	.61	.23	.75	.65	-.19	.21	.47	.65	-.50
Macedonia	.19	-.17	.04	-.07	.82	.65	.71	.26	-.27	.07	.11	-.00
Vojvodina	.76	.39	.70	.86	.80	.12	.49	-.57	-.39	.58	.69	-.02
Croatia	.42	.34	.51	.60	.69	.26	.18	-.26	-.10	.39	.31	-.13
Slovenia	.60	.48	.64	.65	.44	.60	.29	-.32	.40	.60	.53	-.22

Appendix C

Census data, including age distribution, nationality, and educational levels of the county populations are taken from various volumes of the multivolume final census reports of the Federal Institute for Statistics (Savezni Zavod za Statistiku). The 1971 census results were published by the institute under the series title *Popis stanovnistva i stanova, 1971* [Census of the population and apartments, 1971] in Belgrade in 1974. The 1981 census results were published in Belgrade during 1983 and 1984 by the institute under the series title *Popis stanovnistva, domacinstava, i stanovnistva i stanova, 1971* [Census of tion, households, and apartments in the year 1981].

Some economic data, sectoral employment data, data on the number of doctors and dentists, and data on television ownership are reported in the annual statistical handbooks of the institute, entitle *Popis stanovnistva, domacinstava, i* [Statistical yearbook of the SFR of Yugoslavia], usually with a one- to three-year lag. English translations of the introductory materials, table headings, and labels are available for each of these volumes. Other economic data and newspaper circulation data for 1980 are published in the institute's periodical publication *Statisticki bilten* [Statistical bulletin], which appears about 40 times a year and reports a wealth of social, political, economic, and cultural data.

Each of these publications contains definitional materials for the data reported.

Notes

1. It would seem to be far more promising to use aggregate data on membership in the ruling party for this purpose. However, even use of these data, were they available, would require careful specification of the social and political meaning of the decision to join the Party. For example, does it reflect primarily "regime support" or an attempt to advance one's career prospects? Is party membership a mass or an elite phenomenon? In any event, we were unable to obtain such data in a statistically useful form.

2. See, for example, the series of exchanges in the following: Dusan Bilandzic's interview in *Vjesnik*, 8 May 1982; the commentary, "Suspect Yugoslavs," in *Politika*, 13 May 1982; the summary of reactions to the Bilandzic interview in *Politika*, 22 May 1982; and the article by Bilandzic in *Politika*, 23 May 1982. For empirically based discussion, see the studies Vuskovic 1982 and Raic 1982 and the telephone survey reported in "Otkud 'plima jugoslovena'?" [Whence "the tide of Yugoslavs"?], *NIN*, 23 May 1982, pp. 18–21.

3. The exact formula is

$$\text{Fragmentation} = 1 - \sum_{i=1}^{n} \left(\frac{f_i}{N} \right)^2$$

where f_i/N equals the proportion of individuals in the county in nationality group i (Rae and Taylor 1970).

4. Lichtman, for example, argues that "estimates of individual behavior from information pertaining to aggregate units are not necessarily less useful than estimates from data pertaining to smaller units or even to individuals." He goes on to suggest that "if aggregation tends to randomize independent variables, it can reduce specification error and facilitate the goal of portraying the independent effects of individual behavior. Moreover, historians will frequently be able to measure a significantly greater number of relevant variables at higher levels of aggregation. For instance, a much wider variety of statistical measures is generally available for counties than for voting precincts. As a result, estimates derived from county-level data may be more useful than estimates derived from precinct-level data. A well-specified model at a higher level of aggregation may yield more precise and reliable estimates of the direct influence of independent variables than a poorly specified model at a lower level of aggregation" (Lichtman 1974, 432).

5. Separate analyses might also be carried out for urban and nonurban areas of the country. Unfortunately, socioeconomic and demographic data comparable to those available for counties cannot be compiled for individual urban areas, as distinct from the counties of which they are a part. Urbanization, however, may be conceived as a process that subjects populations to certain social processes. From this perspective, "urbanicity" can be viewed not just as the concentration of population but as the concentration of both the working-age and more highly educated populations, in particular; as greater exposure to newspapers and (to a lesser extent) television; as greater access to social services, including medical care; and, finally, as increased intergroup contact.

These social processes are reflected in the strong correlations displayed in Appendix B. They may be said to be the politically most salient dimensions of urbanization, while the process of establishing the physical infrastructure characteristic of urban places is simply a reflection of the level of material development. The proportion of apartments in each county linked to a public sewerage system in 1981, for example, may be considered a strong indicator of the presence of such infrastructure. It correlated much more strongly with the overall level of development of the commune (as measured by per capita income in 1980) than with its 1981 population density. Even when density was held constant, this relationship remained strong. Moreover, it held in every region, both developed and underdeveloped. Thus, rather than grouping the cases into urban and nonurban counties, we accounted for the effects of urbanization by focusing on its politically salient component processes, as measured by the relevant explanatory variables.

6. The multiple linear regression model for a dichotomous (0,1) dependent variable is termed the linear probability model, and this model is often compared unfavorably with other techniques. Criticisms of it concern (1) the lack of efficiency (minimum variance) of its coefficient estimates, which is obviated by shifting from ordinary least squares to weight least squares estimation; and (2) the possibility of out-of-range (i.e., outside [0,1]) linear predictions, which is obviated by employing a nonlinear function that is bounded between 0 and 1 (e.g., logistic). However, if the data are aggregated so that the dependent variable becomes the proportion of cases in each group exhibiting the behavior of interest (i.e., the proportion who declare Yugoslav identity in each county), "so long as the sample sizes in each cell are sufficiently large, and provided $\hat{p}_i$ is not equal to 0 or 1, *the linear probability model is not subject to the criticisms made earlier* (Maddala 1983, 29; emphasis added). The complete census count for a Yugoslav country is certainly a large enough sample size, and our predicted proportions using the linear probability model are within range. Our coefficient estimates are not fully efficient owing to mild heteroscedasticity, but that hardly matters in the context of a "sample" consisting of the entire population of counties, in which virtually the entire population of individuals was contacted. Thus, we present results throughout this paper based on the linear probability model for aggregate data estimated via ordinary least squares estimation.

7. Another approach to determining the cross-time validity of the findings might be to develop a dynamic model, using changes over time in the values of our explanatory variables to explain changes in the proportion of "Yugoslavs." However, while there has been substantial change over the period 1971–81 (for example, in the level of material and social development of the counties and in the proportion of the population with higher education), very strong rank order correlations between 1971 and 1981 values of our predictor variables suggest that the *relative* positions of the counties on these indicators have remained substantially *un*changed. These two approaches, therefore, yielded essentially the same results with respect to identifying important explanatory variables.

8. Data not shown. Regression tables available on request.

9. Jug Grizelj, "Koliko ste zadovoljni radom SIV" [How satisfied you are with the work of the FEC], *NIN*, 20 April 1986; Ljuba Stojic, "Na granici odricanja" [At the limit of self-denial], *NIN*, 10 October 1987.

10. Marinko Culic, "Peticionasi brisu ustav" [Petitioners erase the constitution], *Danas*, 29 March 1988; idem, "Kakav ustav hoce pisci?" [What kind of constitution do the writers want?], *Danas*, 3 May 1988.

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RESEARCH NOTES

IDEOLOGICAL VALUES AND THE VOTES OF U.S. SUPREME COURT JUSTICES

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It is commonly assumed that Supreme Court justices' votes largely reflect their attitudes, values, or personal policy preferences. Nevertheless, this assumption has never been adequately tested with independent measures of the ideological values of justices, that is, measures not taken from their votes on the Court. Using content analytic techniques, we derive independent and reliable measures of the values of all Supreme Court justices from Earl Warren to Anthony Kennedy. These values correlate highly with the votes of the justices, providing strong support for the attitudinal model.

The fundamental assumption about the behavior of Supreme Court justices—that their votes are strongly dependent upon their attitudes, values, or personal policy preferences—has never been adequately tested with *independent* measures of the attitudes or values of justices.[1] We use a systematic content analysis to derive reliable measures of the ideological values of Supreme Court justices that are independent of the votes they later cast. Despite potential problems of measurement error in the data, our scores correlate quite highly with the votes cast by the justices in civil liberties cases from 1953 through 1988.

Attitudes, Values, and Judicial Behavior

The study of judicial behavior begins with C. Herman Pritchett's *The Roosevelt Court* (1948). The book systematically examines dissents, concurrences, voting blocs, and ideological configurations from the nonunanimous decisions rendered by the justices from 1937 to 1947. Pritchett does not provide a theory of Supreme Court decision making; there is no doubt a limit to how many breakthroughs a single book can make. Nevertheless, the theoretical assumptions of the study were made clear in the introduction: "This book, then, undertakes to study the politics and values of the Roosevelt Court through the nonunanimous opinions handed down by its justices" (p. xii), acknowledging that the justices are "motivated by their own preferences" (p. xiii).

It is Schubert (1965), drawing most heavily on the work of Coombs (1964), who first provides a well-delineated theory of judicial decision making. According to Schubert, justices have ideal points (*i*-points) in multidimensional, ideological space. These points represent the justices' "own syndrome of attitudes" (Schubert 1965, 27). Case stimuli (*j*-points), presented to the justices for approval or disapproval, can similarly be measured in the same multidimensional space: "The decision of the Court in any case will depend upon whether the case dominates, or is dominated by, a majority of *i*-points" (p.38).

Rohde and Spaeth (1976) also develop a multidimensional attitudinal model. Their most basic assumption is that "each member of the Court has preferences concerning the policy questions faced by the Court, and when the justices make decisions they want the outcomes to approximate as nearly as possible those policy preferences" (p. 72). The Court's structure grants the justices great freedom "to base their decisions *solely* upon personal policy preferences for a number of reasons: (1) the lack of electoral accountability, (2) the lack of ambition for higher office, and (3) the fact that the Supreme Court is the court of last resort" (p. 72; emphasis added).

The attitudes and values described in these works are taken from the votes cast by the justices. While there is much to learn from this approach—these books are rightly placed among the leading ones in judicial behavior—there are limits as well. One cannot demonstrate that attitudes affect votes when the attitudes are operationalized from those same votes. As Schubert himself recognized, "The book is full of talk about the attitudes of Supreme Court justices, but the attitudinal differences delineated and denoted obviously are hypothetical rather than empirical constructs, because the data analyzed are based on observations of judicial votes in the decisions of cases—and not even on judicial responses to questionnaire items" (1974, xii).

There is only one previous effort to derive independent measures of the values of Supreme Court justices: Danelski's 1966 examination of Justices Brandeis and Butler. Danelski content-analyzed speeches given by the justices prior to their appointment to the Court. Support for or opposition to laissez faire correlated with the direction of solo dissents in economic cases by these two justices. While this work is innovative, the reliability of the data is unknown, and only two justices are studied. Independent evidence for the attitudinal model must rest on a firmer foundation.

Greater efforts have been made to examine the effects of attitudes on the decisions of lower court judges, often through the use of surveys and questionnaires. Nagel (1963) found differences between liberal and conservative judges across four separate issues. Unfortunately, the presentation of the results in terms of the percentage of judges above or below court averages makes it difficult to assess the strength of the relationship. Gibson (1978) found that attitudes alone could explain only 14% of the variation in the decisions of Iowa trial court judges. Nardulli, Flemming, and Eisenstein (1984) report a negative bivariate relationship between a judge's belief in punishment and plea-bargained sentences. In their multivariate model the relationship remains negative for crimes that are not serious, and is positive only for the most serious crimes.

These low or negative correlations should not be surprising given what is known from the social psychology literature. The earliest research in social psychology found little or no relationship between attitudes and behavior. Wicker (1969) concluded from his review of numerous studies that there is little relationship between the two. More recently, however, scholars have found some relationships between attitudes and behavior and have shifted their focus from whether attitudes affect behavior to the conditions under which attitudes affect behavior (see McGuire 1985; Petty and Cacioppo 1981). For example, relationships are enhanced when subjects' thoughts are internally focused (Scheir and Carver 1980) and when attitudes are formed as the result of direct experience (Fazio and Zanna 1981). Relationships are strongest for those high in self-esteem (Sjoberg 1978) and maturity (Henschel 1971). Nevertheless, even when relationships are found, attitudes typically explain only 10% to 40% of the variation in behavior (Petty and Cacioppo 1981, 24–27).

Despite these results, the freedom given to Supreme Court justices to act upon their ideological values (Rohde and Spaeth 1976) gives hope that strong correlations can be found between independent measures of their values and their votes on the Court. We turn now to our efforts at measuring the values of the justices.

The Justices' Ideological Values

Acquiring independent measures of the ideology of Supreme Court justices is not a simple task. As Tate stated,

> It is difficult to imagine what independent measures of judicial values could be used. . . . Attitude surveys, often used to measure the values (and role perceptions) of sitting lower court judges, have their limitations when most of the justices being analyzed are dead. An alternative method, inferring values from written works of judges (Danelski 1966), might be applicable in principle, but would require a research investment which is well beyond the resources of most researchers if applied to 25 justices. (Tate 1981, 365)

As part of a separate study we conducted a content analysis on the ideological values of all justices from Earl Warren to Anthony Kennedy from newspaper editorials in several of the nation's leading newspapers (Segal, Cover, and Cameron 1988). Although these measures must be used with caution, we believe that such measures can be justified under certain circumstances. First, though, we describe how the measures were obtained.

We started by conducting a content analysis of a source that contains comparable information on each justice since Earl Warren: statements in newspaper editorials from the nomination by the president until the confirmation vote by the Senate. Newspaper editorials are one of the few independent sources of information that contain comparable data on each justice. Only editorials prior to confirmation by the Senate were examined, for editorials after confirmation will undoubtedly be influenced by votes a justice casts and thus are not independent of those votes. We selected four of the nation's leading papers, two with a liberal stance (the *New York Times* and *Washington Post)* and two with a more conservative outlook (the *Chicago Tribune* and *Los Angeles Times).*

To conduct the content analysis, we trained three students to code each paragraph for political ideology. Paragraphs were coded as *liberal, moderate, conservative,* or *not applicable.* Liberal statements include (but are not limited to) those ascribing support for the rights of defendants in criminal cases, women and racial minorities in equality cases, and the individual against the government in privacy and First Amendment cases. Conservative statements are those with an opposite direction. Moderate statements include those that explicitly ascribe moderation to the nominees or those that ascribe both liberal and conservative values.[2] The justice's ideology (JI) is then measured by the formula JI = (liberal − conservative) / (liberal + moderate + conservative). This formula leads to a scale ranging from +1.0 (unanimously liberal) through .0 (moderate) to −1.0 (unanimously conservative).

The measures, presented in Table 1, are reliable. We assessed reliability by using all three coders on a 25% random sample of the data. Using pi as our index of intercoder reliability (see Krippendorff 1980), a result of .72 was achieved. This is significant at $p < .001$.

We believe that the scores accurately measure the perceptions of the justices' values at the time of their nominations. While not everyone would agree that every score precisely measures the perceived ideology of each nominee, Fortas,

Table 1. Justices' Values and Votes

Justice	Values[a]	Votes[b]
Warren	.50	78.1
Harlan	.75	41.9
Brennan	1.00	77.9
Whittaker	.00	43.4
Stewart	.50	51.5
White	.00	43.4
Goldberg	.50	89.6
Fortas	1.00	80.4
Marshall	1.00	79.7
Burger	-.77	29.7
Blackmun	-.77	42.9
Powell	-.67	37.9
Rehnquist[c]	-.91	19.5
Stevens	-.50	56.3
O'Connor	-.17	30.9
Rehnquist[d]	-.91	23.0
Scalia	-1.00	34.7
Kennedy	-.27	40.0

[a] Derived by authors. The range is -1.00 (extremely conservative) to 1.00 (extremely liberal).

[b] Percentage liberal in civil liberties cases, 1953-88.

[c] Values and votes as Nixon appointee.

[d] Values and votes as Reagan appointee.

Marshall, and Brennan are, expectedly, the most liberal, while Scalia and Rehnquist are the most conservative. Harlan and Stewart come out liberal because the debate about them centered around their support for the overriding issue of the day, desegregation. Goldberg is not perceived to be as liberal as Fortas or Marshall because of an even-handedness at the Department of Labor that even the *Chicago Tribune* could support. O'Connor comes out a moderate, given her previous support for women's rights and abortion. Indeed, the only hint of opposition to her came from the Right.[3]

Measures of *perceived* values are obviously imperfect measures of the *real* values of Supreme Court justices. In a world more attuned to the needs of researchers, Supreme Court justices would annually complete attitude questionnaires on each issue the Court would consider during the year. Neither this nor a single questionnaire completed at each justice's appointment are likely to happen soon. We must turn instead to indirect methods of measuring the ideological values of justices. Some form of content analysis is needed. The criteria that led us to choose newspaper editorials follow. First, the data must have ideological content. Second, the data must be comparable; the data must exist in similar form for each justice. Third, the data must be independent of the votes the justices cast; the data cannot be rationalizations of votes already cast. Fourth, there can be no systematic errors in the data.

We considered but rejected several possibilities. Coding cases decided by the justices while on lower courts would not work because many justices do not have lower-court experience. Opinions written by the justices are clearly not independent of votes previously made. Speeches made or articles written by justices, when they have ideological content, may be rationalizations or defenses of decisions previously cast. These writings and speeches are independent only of votes not yet cast. Preappointment articles and speeches by justices would be appropriate for analysis, but too many justices have no articles or speeches published prior to selection for the Court. Further, while some justices, such as Douglas (1970), write and speak on ideologically charged subjects, others—for example, Scalia (1976)—write on topics such as oversight of agency decision making.

Senate confirmation hearings often have ideological content and contain comparable data for the overwhelming majority of justices. However, there is likely to be systematic bias in such data. When a nomination is noncontroversial, we are likely to get little or no ideological information from the nominee's statements. O'Connor, for example, thought it in her best interest to refuse to answer virtually all policy questions. When a nominee does agree to address such questions, it is usually done in an attempt to satisfy senators that the nominee is not as liberal or conservative as commonly believed. Thus, if we were to code Bork's statements before the Senate Judiciary Committee, we would find Bork consistently stating his support for women's rights and First Amendment rights, precisely because senators had reasons to believe he opposed them. Therefore, the data from confirmation hearings may well be biased.

Newspaper editorials on nominations to the Supreme Court exist on all nominees since Earl Warren, typically have ideological content, and are obviously independent of the votes justices later cast. While editorials presumably are quite accurate in conveying preconfirmation perceptions of the ideology of nominees, these perceptions are not necessarily accurate. For example, our measures suggest that Harlan is a liberal and O'Connor is a moderate. Few would argue that this represents their true values. What effects would such errors have on our analysis? First, we argue that such errors are random. Prior to the editorials' publication, there is no reason to believe that any nominee will be systematically misrepresented in one direction or another. Compare this to statements from judiciary committee hearings, where the errors are systematic—liberals under intense scrutiny try to portray themselves as less liberal, conservatives are less conservative. Second, to the extent that we have random measurement error, we will undoubtedly find weaker correlations than would otherwise be the case (Berry and Feldman 1985). Therefore, the correlation between ideological values and votes that we present is lower than the true correlation. Under such circumstances, if we find low correlations, we cannot state that values are unrelated to votes. If high correlations are found, we will have found the *minimal* level existing in the true relationship.

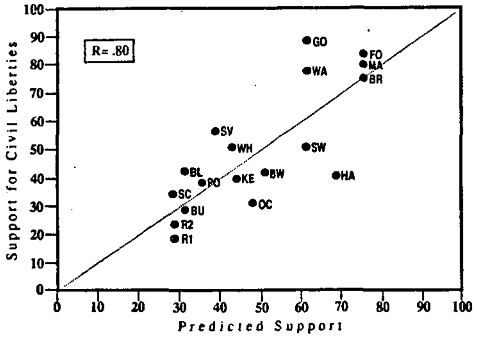
The Dependent Variable

Because the statements in newspaper editorials deal almost exclusively with support by the justices for civil liberties and civil rights, we use as our dependent variable the votes of all justices appointed since the beginning of the Warren Court in all formally decided civil liberties cases from the beginning of the 1953 term through the end of the 1987 term, as derived from the Supreme Court data base compiled by Harold Spaeth at Michigan State University.[4] Civil liberties issues are those involving criminal procedure, civil rights, the First Amendment, due process, and privacy. Liberal decisions are (1) pro-person accused or convicted of crime, (2) pro–civil liberties or civil rights claimant, (3) proindigent, (4) pro-Indian, and (5) antigovernment in due process and privacy. The data are presented in Table 1.

Results

The results are straightforward: the correlation between the ideological values of the justices and their votes in civil liberty cases is .80 (r^2 = .64, adjusted r^2 = .62). Regressing votes on our measure of values yields a constant of 51.25 and a slope of 23.44 (t = 5.34). The residuals are graphed in Figure 1. The largest residuals belong to Harlan, who is 27.3 percentage points more conservative than expected, and Goldberg, who is 26.4 percentage points more liberal than expected. Brennan, Marshall, Burger, Powell, and Kennedy are within 5 points of predicted values; Whitaker, White, Fortas, and Rehnquist (R1) are within 10. Also within 10 points are Rehnquist (R2), Scalia, and Kennedy, who fit well despite the relatively few votes they have cast. Regression diagnostics demonstrate that the results are remarkably stable; they are not dependent on the scores of any particular justice. Systematically excluding one justice at a time results in correlations

Figure 1. Civil Liberty Support by Ideology



Note: WA = Warren, HA = Harlan, BR = Brennan, WH = Whittaker, SW = Stewart, BW = White, GO = Goldberg, FO = Fortas, MA = Marshall, BU = Burger, BL = Blackmun, PO = Powell, R1 = Rehnquist (1971), SV = Stevens, OC = O'Connor, R2 = Rehnquist (1986), SC = Scalia, KE = Kennedy.

ranging from .77 (Justice Marshall excluded) to .86 (Justice Harlan excluded). The correlation is unchanged if Justice Kennedy's 40 votes are excluded. Given the limits on the relationship between attitudes and values, and the fact that our correlation is attenuated by the measurement error that no doubt exists in the independent variable, the results provide exceptional support for the attitudinal model as applied to civil liberties cases. For the first time support is from measures of values independent of the votes of the justices.[5]

We observe a correlation of .80 between values and votes. If the true correlation is significantly higher than that, does this mean that virtually nothing else directly affects the aggregate decisions of justices?[6] Traditional modes of analyzing judicial decisions emphasize the importance of legal doctrine and precedent. This is not the place for a complete defense of legal realism, but we do briefly note the following. Supreme Court justices are not bound by the legal doctrines accepted by the Court majority; they are free to use whatever doctrines fit their own preferences. Precedents are typically found on both sides of any case reaching the Supreme Court; and even if the precedents weigh heavily on one side, justices are free to distinguish or overrule them. While precedent might have some value for some justices, the empirical evidence on the importance of precedent consists of little more than Schubert's (1963) exposition of the votes of Justice Clark in courts-martial of civilian personnel and dependents. Gibson (1978) demonstrates the importance of roles—particularly as to judicial restraint—to Iowa judges, but Supreme Court justices need not feel any more constrained by judicial restraint than they do by precedent. Evidence on the Court establishes that judicial restraint is little more than a "cloak for the justices' policy preferences" (Spaeth and Teger 1982, 277; see also Spaeth 1964).

Case facts obviously influence Supreme Court decisions (Segal 1984, 1986), but facts can only explain variance between cases of a particular Court or between the decisions of a particular justice; they cannot explain differences in decisions between justices. That is, fact models try to explain why Justice *A* votes liberally in one case and conservatively in another; attitudinal models try to explain why Justice *A* is moderate over a series of cases while Justice *B* is liberal. Attitudes must have a referent. In the attitudinal model of judicial behavior that referent is the case stimuli, or in Schubert's term, *j*-points. There is no contradiction between the models. They necessarily differ only because the units of analysis differ.

The attitudinal model also fails to consider internal and external influences on the Court. Walter Murphy's seminal *Elements of Judicial Strategy* (1964) documents innumerable methods for successfully "marshaling the Court" from within. External influences are also likely to exist. The solicitor general is remarkably successful at the certiorari stage (Tanenhaus et al. 1963) and when cases are decided on the merits (Puro 1971, 1981; Scigliano 1971; Segal 1988). This influence exists, at least in sex discrimination cases, even after the facts of the cases have been controlled (Segal and Reedy 1988). So while justices may have a free reign to vote their personal policy preferences, there are forces that limit this discretion. Nonetheless, the ability of the attitudinal model to explain the justices' voting behavior indicates that these influences are minimal. As better measures of the values of the justices are developed across multidimensional space, the limitations of alternative explanations of the justices' aggregate behavior should become ever more apparent.

Notes

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1. We recognize the existence and importance of the Danelski 1966 study, and discuss it in further detail later. Nevertheless, as his study examined but two justices, we cannot consider it a test of the attitudinal model.

2. We provide here a few examples of the types of statements that would lead to a paragraph being coded liberal, moderate, or conservative.

Liberal: "Scarcely more defensible were the numerous questions about Judge Harlan's affiliation with the Atlantic Union. The country would have a sorry judiciary indeed, if appointees were to be barred for belonging to progressive and respectable organizations" *(Washington Post,* 2 March 1955) and "During the McCarthy era he [Fortas] was not afraid to raise a strong voice against those who were bent upon destroying civil liberties under the banner of anticommunism" *(Washington Post,* 30 July 1965). *Moderate:* "On the contrary, Powell's reputation is as a moderate by Southern standards on race, as a man with deep and humane concern for his fellowman" *(Los Angeles Times,* 24 October 1971). *Conservative:* "Judge Carswell himself admits to some amazement now at what he said in that 1948 speech. He should, for his were the words of pure and simple racism" (*Washington Post,* 10 February 1970) and "He [Nixon] has strongly supported Rehnquist's articulation within the Justice Department of broad authority for surveillance and wiretapping in the pursuit of criminals, and in the qualified form of martial law used during the May-Day demonstrations" *(Los Angeles Times,* 5 December 1971).

3. Rehnquist is included twice, once as associate justice and once as chief justice. While his ideological scores are the same both times, the scores are separate measures, taken from 1972 and 1986, respectively. That they are identical down to two decimal places is partially a coincidence, but mostly a result of the fact that he was viewed as a strong conservative both times.

4. The paucity of editorials prior to Warren prevents us from beginning with earlier justices. Since the data base begins with the appointment of Earl Warren, we have the complete voting record on all included justices.

5. Or one could suggest the following alternative scenario: If judges base decisions on legal values (e.g., precedent, intent of the framers, etc.), not personal values, editorials on nominees with lower court experience would be based on those legal values. Our measures will correlate with their latter votes on the Court, because as justices they will again be basing their votes on their legal values. But if this were the case, lower court experience would provide crucial information that does not exist for those without such experience: the legal values of the nominees. The correlation for those with that information should be higher than the correlation for those without that information. Though the n's are small, the correlation between values and votes for those with judicial experience is .76 (adjusted r^2 = .53), while the correlation for those without such experience is .92 (adjusted r^2 = .81). While this is not sufficient to claim that lower court experience provides *disinformation* (one might reasonably argue that lower court judges are bound by stare decisis, while Supreme Court justices are free to follow their policy preferences), it is clear that lower court service provides no special evidence about how justices will vote, contrary to the implications of legalistic explanations.

6. The findings presented here do not foreclose the possibility of indirect effects on the justices' decisions, such as personal attributes (see Tate 1981), which may influence decisions by affecting the justices' values. Our data enable testing of the first link of the model (attributes affect values) and our results demonstrate the validity of the second link (values affect votes).

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Votes of Supreme Court Justices

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ECONOMIC CONDITIONS AND THE PRESIDENTIAL VOTE

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This analysis demonstrates that the relative growth of per capita income change is an important determinant of post–World War II presidential election outcomes. Per capita income change is even a better predictor of presidential election outcomes than the electorate's relative attraction to the Democratic and Republican candidates as calibrated in National Election Study surveys. The significance of this finding is discussed.

The state of the economy is widely believed to be a major determinant of presidential election outcomes. For supportive evidence, statistical analyses of the post–World War II time series do show a correlation between economic conditions and the incumbent party vote. Tufte (1978, 120–22) reports a correlation of .64 for the eight elections of 1948–76 between the election year growth in per capita income and the incumbent party vote. Using a more refined measure of income change, Hibbs (1987, 195–200) reports an even higher (and statistically significant) correlation of about .80 for 1952–80. Tufte's measure also achieves statistical significance when a control is added for "net candidate advantage" (net candidate likes and dislikes from the National Election Study [NES] data for that election). (See also Fair's [1978, 1988] more complex model based on presidential elections since 1916.)

Impressive as this evidence is, the statistical case for economic effects in presidential elections may seem more suggestive than persuasive. The cautious observer is tempted to discount a correlation based on a very small number of electoral observations (1988 is the eleventh since World War II), especially since so many additional variables besides the economy are believed to influence presidential outcomes. (For one perceptive discussion, see Rosenstone 1983, 40–42.)

My purpose is to demonstrate unmistakable evidence of the impact of economic conditions on presidential elections, which is far greater than is generally realized. For this exercise, I extend the time series of presidential elections from 1948 through 1984. As an important statistical control, I employ Tufte's measure of net candidate advantage. For the strongest estimates, I use Hibbs's measure of income change, adding an estimate for the 1948 observation. Hibbs's measure is preferable because it represents the cumulative lagged effect of personal disposable income over all but the final three months of the presidential administration. This choice is not crucial, however. Tufte's measure of income change for the election year is a servicable measure that predicts almost as well. (The two measures correlate at .92 for the 1948–84 observations.) Alternative specifications with different choices of variables and choices of election years are explored in the Appendix.

The Equation

For the 10 presidential elections of 1948–84 the equation is

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$$V = 44.64 + 2.77 \bullet (I) + 6.50 \bullet (C) + e,$$

standard error of b	.52	1.51
t-ratio	5.28	4.31
significance level	.001	.004
standardized b	.63	.52
simple r with vote	.83	.75
partial r	.89	.85

Adjusted R^2 = .888
Standard error of the estimate = 2.21
Durbin-Watson = 2.10

where V = incumbent presidential party's percentage of the two-party presidential vote; I = cumulative weighted average of annual percentage change in per capita disposable income over the previous four years, based on data for 15 quarters, with recent quarters weighted most heavily (Hibbs 1987, 194);[1] and C = per respondent net candidate advantage, based on NES respondent likes and dislikes about the presidential candidates' personal characteristics (Tufte 1978, 121; corrected and updated through 1984 using NES data).[2] Four aspects of the equation are remarkable:

1. The two independent variables together account for almost 90% of the variance in presidential election outcomes. Little is left to explain from other variables.
2. Even with only 10 cases, the coefficients for income change and candidate evaluation are extremely significant (at .001 in the case of income change!). The high explained variance helps to account for this: each of the two central predictors explains the variance that the other does not.
3. As measured by the sizes of the standardized coefficients, the two central predictors account for presidential outcomes in about equal amounts. Income change in fact has the slight edge. One can better predict the presidential vote division from income change during the previous administration than from the voters' relative liking for the two candidates!
4. In the literature on economic voting, the high-side estimate of the effect of annual income change is about one percentage point of the vote for every annual percentage increase in per capita income (Kiewiet and Rivers 1985a). The estimate here (2.77) exceeds this consensus estimate by almost a factor of three.

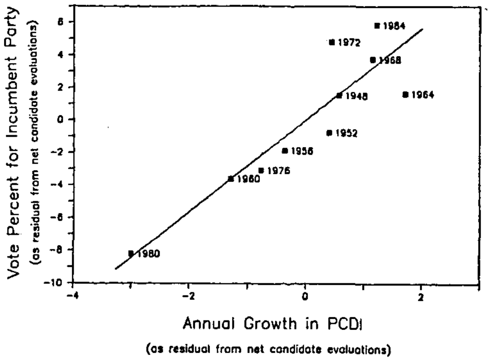
How well does per capita income growth predict elections? One might be tempted to answer this question by observing the beta, or standardized coefficient for income change. But this test would be misleading. The standardized regression coefficients for income change and candidate advantage are actually lower than the zero-order correlations. This is because standardized coefficients run into a statistical ceiling: the sum of the products (beta) • (r) must equal the R-squared, which of course cannot exceed 1.00.

The importance of income change can best be seen from the partial correlation, a relatively underused statistic. The partial correlation between two variables is the correlation between the two variables' residuals from regression equations predicting the variables from the controls. For candidate advantage, the partial correlation with the vote (controlling for income change) is an impressive .85. This is reassurance that elections tend to be won by the candidates that the voters like best. But the partial correlation between income change and the vote (controlling for candidate advantage) is an even stronger value of .89! This convincing correlation is shown in Figure 1. Evaluations of candidate personal characteristics aside, the vote is determined almost entirely by the amount of prosperity that the incumbent party delivers.

The Economy vs. the Campaign?

Typically, scholars discuss different explanations for a phenomenon as if the explanations were in competition with one another: if one explanation is strengthened, the other, by implication, is weakened. Here we see that economic conditions matter considerably, just as many observers have suspected. But does this evidence for "economic determinism" disturb the traditional paradigm of voting studies, which posits a strong role for candidates and campaign events? The answer, of course, is *not at all.* As summarized by the electorate's net candidate evaluations, candidates and campaigns matter about as much as the state of the economy. We are left, therefore, with a multivariate explanation. (See also Markus 1988.)

Figure 1. Presidential Vote by Per Capita Income Growth



So far I have neglected the matter of indirect effects. In addition to its direct effect, the state of the economy could also exert an *indirect* effect via the net candidate advantage. The explanation might be a presidential-level version of Jacobson and Kernell's (1981) "strategic politicians" theory: the economy determines the parties' prospects, which determine the quality of their candidates and the vigor of their campaigns. Also, voters could evaluate the candidates through the filter of the state of the economy. Given these plausible arguments, the actual correlation between these two independent variables is rather low, only .37. A correlation this small means little with only 10 cases. (The adjusted R-squared, for instance, is only .03.) The net candidate advantage, therefore, is a "wild card" influence on the election that cannot be forecast in advance even if one knows how the state of the economy will play out through the year of the election.

The Reagan Elections and Economic Voting

To a considerable extent, the Reagan elections account for the strength of the model. Reagan won in 1980, even though the NES net candidate advantage went to President Carter. Reagan, of course, benefited from the unprecedented election year economic downturn. In 1984 Reagan won in a landslide even though his net candidate advantage over Mondale was only slightly greater than Carter's over Reagan four years before. (Reagan's supposed popularity and Mondale's supposed unpopularity do not register strongly in the NES candidate likes-dislikes questions; see Appendix, Table A-2.) The strong preelection prosperity, of course, accounts for the 1984 outcome. This interpretation fits nicely with the school of thought which holds that observers too readily attributed Reagan's electoral success to his personal appeal and the appeal of his conservative ideology and underestimated his good economic luck (Ferguson and Rodgers 1986; Hibbs 1987; Kiewiet and Rivers 1985b).

"Forecasting" 1988

How well did the model do in 1988? As of this writing, the NES 1988 Election

Study is not yet available for analysis. Thus, the 1988 net candidate evaluation cannot be included in the 1988 "forecast." But the appropriate economic statistics have been reported. Using Hibbs's algorithm for giving greater weight for quarters close to the election, per capita income grew at annual rate of 2.1% in 1985–88 (2.3% in 1988 alone).

Contrary to the most exaggerated claims of how prosperity made a Republican victory in 1988 all but certain, the 2.1% rate of income growth for 1988 was below par (see Appendix, Table A-2). Suppose we plug the 2.1% grow rate into the forecasting equation while assuming an even balance of zero for the net candidate advantage. This gives a 1988 "forecast" of a 50.4% Republican vote, a margin "too close to call."

Although Bush obtained about four percentage points more of the vote than "predicted" from the economy alone, the difference can readily be attributed to the unmeasured net candidate evaluation. Since campaign polls showed a clear Bush advantage over Dukakis in popularity and approval (variously measured), almost certainly the NES net candidates scale shows the same differential. A net candidate advantage on the order of Eisenhower's over Stevenson in 1952 (or half Eisenhower's larger edge in 1956) would be enough to predict the 1988 outcome almost exactly.

Thus, the story of 1988 seems to be as follows: The actual degree of economic prosperity was less than commonly supposed. In fact, the degree of prosperity was at the margin where it was insufficient to forecast the presidential winner without additional information. A more popular Republican than Democratic candidate—not the economy—was the decisive factor. Still, because the 1988 data point appears to fit the equation, the 1988 result in no way diminishes the interpretation that the economy contributes greatly to presidential outcomes.[3]

Economic Voting and the Theory of Elections

The major purpose of the statistical model is to advance the theory of election outcomes rather than to make predictions. The model demonstrates that the state of the economy and the electorate's evaluations of the candidates together but independently drive presidential election outcomes. That the economy can match candidate evaluations in importance is an unexpectedly strong result. Consider that presidential elections tend to be determined by the electorate's relative liking of the two candidates but that independent of this almost tautological effect, the state of the economy is an even-better predictor of the outcome. More than ever, there appear to be grounds for investigating the process that causes individual voters to respond to the nation's recent economic performance (Fiorina 1981; Kiewiet 1983; Markus 1988). And more than ever, we see evidence of a rational incentive for the party in power to manipulate the business cycle for electoral benefit.

Appendix

Table A-1 presents some of the results of alternative specifications of the regression model. As the base, column 1 reports the equation discussed in the text.

Column 2 shows the result omitting candidate evaluations (C). Omitting candidate evaluations allows the coefficient for income change to reflect both direct effects and indirect effects via candidate evaluations. Removing C from the equation increases the coefficient for income change (*I*) but at the cost of a larger standard error and lower t-ratio.

Column 3 shows the result of substituting Tufte's measure of annual income change (*I*′) for Hibbs's (*I*) for 1948–84. Tufte's measure yields a lower coefficient for income change and a higher coefficient

Table A-1. Alternative Equations Predicting the Incumbent Party Presidential Vote, 1948–84

						1948–76 Only		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hibbs PCDI Growth (I)	2.77 (.52)	3.60 (.87)		2.87 (.49)		2.46 (.88)		2.93 (.80)
Tufte PCDI Growth (I′)			1.80 (.32)		1.81 (.45)		1.31 (.79)	
Candidate Evaluations (C)	6.50 (1.51)		7.20 (1.78)	5.41 (1.57)	6.55 (2.06)	6.77 (1.70)	7.72 (2.09)	7.25 (3.09)
Democratic Incumbent Dummy (D)				-2.19 (1.45)	-1.38 (1.97)			
Presidential Approval (P)								-.04 (.14)
Adjusted R-squared	.888	.641	.836	.905	.823	.867	.779	.871
Standard error of estimate	2.21	3.95	2.67	2.03	2.77	2.30	2.97	2.50
Number of cases	10	10	10	10	10	8	8	10

Note: Standard errors are shown in parentheses. Variables are defined in text.

[a] Annual percentage change in per capita real income, Hibbs measure. See n. 1.

[b] Annual percentage change in per capita real income, Tufte measure. This measure is the percentage change for the election year, based on annual (not quarterly) estimates.

[c] Net candidate advantage. See n. 2.

[d] Incumbent party dummy variable, 1 if Democratic president, otherwise 0.

[e] Presidential approval, last Gallup reading prior to election.

for net candidate advantage. Still, use of Tufte's measure reinforces the message of a powerful economic effect.

Columns 4 and 5 show the results with the addition of a term for the party in power—a dummy variable (*D*) coded 1 for Democratic administrations and 0 for Republican administrations. This variable has a negative (but nonsignificant) coefficient, suggesting the possibility that campaign issues not directly brought into the model tend to disadvantage the Democrats.

The strongest results require inclusion of the two Reagan elections. Columns 6 and 7 show the consequences of their omission. (Column 7 is a replication of Tufte's 1948–76 equation. Differently from this replication, Tufte's original analysis showed an income effect that was modestly significant at the .05 level. I used revised per capita disposable income [PCDI] estimates, and corrected candidate evaluations, which accounts for this discrepancy.)

Finally, column 8 returns to the original model but with presidential popularity added. The final presidential approval reading before the election is known to be an excellent predictor of the outcome, presumably as a surrogate for other variables (Brody and Sigelman 1983; Lewis-Beck and Rice 1982). For 1948–84, this popularity measure correlates at .85 with the incumbent party vote. Could presidential popularity predict so well because it reflects income change and the electorate's net candidate evaluations? As col-

Table A-2. Data

Year	Incumbent Party Presidential Vote	Per Capita Income Change		Net Candidate Evaluations
		Hibbs Measure	Tufte Measure	
1948	52.37	2.58	3.73	.093
1952	44.59	1.80	1.34	-.488
1956	57.76	2.63	2.93	1.025
1960	49.91	1.03	0.15	.376
1964	61.34	4.72	5.47	1.031
1968	49.59	2.60	2.86	-.438
1972	61.79	3.14	2.88	.745
1976	48.89	1.37	2.58	.214
1980	44.70	-.78	-1.09	.279
1984	59.17	3.51	5.80	.358
Mean	52.98	2.26	2.67	.314

Note: Variables are defined in text.

umn 8 shows, with these variables controlled, the coefficient for presidential approval diminishes to insignificance and even becomes slightly negative. This neat result gives added theoretical support for the original model.

Table A-2 presents the raw data.

Notes

The author appreciates the helpful comments of Raymond Duch and Lee Sigelman on an earlier draft of this paper and the research assistance of David W. Romero.

1. Hibbs's measure is the annualized average of the quarterly income growth from the first quarter of the term to the fifteenth. The quarters are weighted so that each quarter is given 1.25 times the weight of the previous quarter (Hibbs 1987, 193, 197, 367–69n). Because Hibbs "OPEC-adjusts" his PCDI estimates, one cannot replicate his measure of cumulative income growth exactly from reported quarterly PCDI estimates.

Hibbs provides estimates for the four-year terms preceding each election, 1952–84 (Hibbs 1987, 193). He does not present an estimate for 1945–48, probably because quarterly readings of PCDI were not introduced until 1947. To obtain a measure of Hibbs's cumulative weighted average for 1945–48, I prorated the pre-1947 quarterly changes based on the annual PCDI readings for 1945 and 1946.

2. The exact candidate evaluations measure is based on as close a replication as possible of Tufte's procedure. I used the mean (for major party voters only) net number of in-party candidate likes and out-party candidate dislikes minus the out-party candidate likes and in-party candidate dislikes. I excluded all issue-based and group-based likes and dislikes. For 1952–76 this index corresponds closely but not exactly ($r = .99$) with Tufte's reported mean candidate evaluations. For 1948 I used Tufte's reported score. (For 1976 it was necessary to correct the sign of Tufte's reported score. Tufte inadvertently assigned Ford's net candidate advantage to Carter.) See also Stokes 1966 and Kagay and Caldeira 1980.

3. We can incorporate the 1988 observation into a regression model predicting the vote from income change alone. This extra data point has virtually no effect on the original equation (see Appendix, Table A-1), adding .01 to the estimated slope and subtracting .01 from the correlation. Adding 1988 as the eleventh case does reduce the standard error of estimate by .19 and the standard error of the regression coefficient by .06.

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CONTROVERSY

LAND INEQUALITY AND POLITICAL VIOLENCE

Considerable research effort has been invested in establishing the appropriate relationship between patterns of land distribution and political violence. In an article in the June 1988 issue of the Review, *Manus I. Midlarsky proposed and tested a new measure of the distribution of land, which he called "patterned inequality." He presented supporting evidence with data from Latin American and Middle Eastern countries. In this controversy, Midlarsky's analysis is challenged by Edward N. Muller, Mitchell A. Seligson, and Hung-der Fu. They advocate an alternative measure of land inequality, test its effect on levels of political violence in Latin America, and find it wanting. In his rejoinder, Midlarsky offers new analytical support for his claims.*

It is frequently asserted that land inequality in agrarian societies is a major cause of rebellion. Cross-national tests of the land inequality hypothesis typically have used the Gini concentration ratio as the operational definition of land inequality. The most comprehensive test to date, conducted with data from 83 countries, found little support for it. A correlation (*r*) of only .24 was observed between the Gini ratio of land concentration circa 1970—weighted by the size of the agricultural labor force—and the death rate from political violence during 1973–77 (Muller and Seligson 1987, 435).

Three studies have recently attempted to uncover the theoretical explanation for the lack of an association between the Gini ratio of land concentration and political violence. Two of these studies insist that land distribution is in fact a major cause of political violence but that the failure to find strong and robust correlations is a function of inappropriate index construction. The third study, our own (Muller and Seligson 1987), views land distribution as important only when linked to the creation and maintenance of income inequality, the variable that has a direct bearing on political violence.

One proponent of the hypothesis that maldistribution of land is a major cause of rebellion argues that the Gini ratio is inappropriate because it is insensitive to the kind of inequality that is most likely to spawn peasant-based insurgency. Prosterman and Riedinger (1987) hypothesize that landlessness, rather than inequality in the distribution of land, is the most important cause of deeply felt grievances among peasants. In their view, the key to predicting the likelihood of large-scale internal violence in agrarian societies is the percentage of landless peasants in the entire population. The Gini land concentration ratio does not speak to the question of landlessness, of course, since it is based only on the landed population.

To support their thesis Prosterman and Reidinger (1987) cite a number of examples of twentieth-century revolutions in societies with substantial proportions of landless peasants (pre-1911 Mexico, pre-1917 Russia, pre-1936 Spain, pre-1941 China, pre-1952 Bolivia, pre-1959 Cuba, and pre-1961 South Vietnam). Although they claim that their index of landlessness is "remarkably well correlated with instances of major instability" (p. 25), Prosterman and Reidinger did not report correlation coefficients and made no effort to determine the predictive accuracy of the landlessness measure. Nor did they conduct tests to see if an association between landlessness and political violence held when other alternative causes of political violence were taken into account. When the 1973–77 rate of death from political violence is regressed on landlessness data circa 1970 (from Prosterman and Reidinger 1982), the adjusted r-squared value is only .04. And when income inequality is controlled for, the association between landlessness and political violence is not statistically significant (Muller and Seligson 1987, 436). Landlessness thus has trivial predictive power in cross-national perspective and is irrelevant also in the multivariate context. In short, it is a variable that does not seem to merit further consideration at this time.

The second proponent of the importance of land scarcity for political violence is Midlarsky (1982, 1988). He argues that a principal cause of political violence in agrarian societies is a process of bifurcation in the pattern of landholdings. Given a fixed supply of land, such factors as high birth rates and the absence of primogeniture among the peasant population will lead to increased subdivision of small farms, and the average size of the smallholders' properties will shrink. If large farms remain relatively intact, it is a classic case of immiserization, in which the rich remain rich while the poor become poorer. Midlarsky (1988) calls this a condition of "patterned inequality" and claims that apart from agreement at the extremes, the Gini concentration ratio is insensitive to differences in patterned inequality and therefore yields low associations with violence. He reports evidence purporting to show that patterned inequality in land distribution is a much more powerful determinant of political violence in Latin America and the Middle East than the Gini ratio.

There are serious flaws in both his measure of patterned inequality and his test of the hypothesis that patterned inequality is a major cause of political violence. Nevertheless, the concept of bifurcation is of real theoretical interest because it suggests an alternative to the equal-share concept of equality that underlies such frequently used measures as the Gini ratio.

The concept of bifurcated inequality, as we will define it here, focuses attention specifically on the difference between the average farm size in the upper and lower extremes of the distribution of landholdings—the gap between rich landlords and poor peasants. Bifurcated inequality is characteristic of the traditional lord-and-peasant type of agrarian society, in which most of the land is concentrated in a few great estates, while there are many very small farms. The condition of bifurcated inequality is reflected only imperfectly by the Gini ratio, which measures the extent to which the cumulative distribution of landholdings departs from a norm of perfect equality and therefore is sensitive to inequalities present in all parts of the distribution.

The hypothesis that bifurcated inequality will be associated with political violence more strongly than is the Gini concentration ratio is predicated on a rejection of the relevance of the equal-share concept of equality as a cause of political violence. Specifically, the assumptions are (1) that owners of middle-size farms will not be unhappy with their share of landholdings even though it may be considerably less than that of large landowners and (2) that owners of small farms will be unhappy with their share only if it is extremely small relative to that of large landowners. Instead of comparing the extent to which the entire distribution of landholdings departs from an equal-share norm of equality, the relevant comparison for measuring bifurcated inequality would be the ratio of the average size of a large farm to the average size of a small farm.

After reviewing the limitations of Midlarsky's patterned inequality measure of bifurcation, we will propose an alternative measure and compare it with Midlarsky's patterned inequality. Then we will conduct tests of Midlarsky's argument that bifurcated inequality is a relevant cause of cross-national variation in rates of political violence in Latin America.

Patterned Inequality

Midlarsky (1988) stipulates that a condition of patterned inequality occurs when the lower end of the distribution of landholdings fits an exponential model and the upper end fits a log-exponential model. The combination of an exponential lower distribution and a log-exponential upper distribution is assumed to be a result of a process of bifurcation in the distribution of landholdings such that the average size of large farms remains stable while that of small farms declines. The exponential-lower-and-log-exponential-upper combination is presumed to represent a high degree of inequality resulting from the process of bifurcation. Distributions that do not conform in the lower part to the exponential or in the upper part to the log-exponential are presumed to represent a low degree of bifurcated inequality.

Midlarsky's strategy of measuring the degree of bifurcation by comparing the fit between theoretical and observed distributions has several limitations. First, it is indirect, based on a set of assumptions about the correspondence between the subdivision of smallholdings and the form of the lower and upper parts of the distribution of landholdings. Since bifurcation, in the sense of the gap between the average size holdings of the rich and poor, is easy to measure directly, there is no need for a measure that merely infers bifurcation from patterns of land distribution.

Second, there are many possible intermediate patterns between the two extreme types, and it is not clear how to rank-order them in regard to differing amounts of bifurcated inequality and differing amounts of expected violence. Midlarsky asserts that a fit of the exponential model to the upper part of the distribution and no fit of either model to the lower portion represents less of a bifurcated inequality than a fit of the log-exponential model to the upper part and no fit of either model to the lower part. But the reason for this difference is not explained. Another intermediate pattern is a fit of the exponential model to the lower part of the distribution and no fit of either model to the upper part. Midlarsky does not try to rank-order this pattern. Still another intermediate pattern involves a "near" fit to one of the theoretical distributions. If near fits are allowed, not only is the number of possible intermediate patterns increased substantially but even the criterion for determining distinct patterns becomes vague, since a near fit in one observer's opinion might not be a near fit to another.

A third problem with the patterned inequality measure is that the definition of the upper and lower ranges of land distribution is not constant or systematically derived across the countries Midlarsky studies. The upper and lower ranges are determined by inspection. This element of subjectivity in the measure introduces the possibility that selection of the data points to be included in the upper and lower ranges could be biased for or against a given hypothesis.

Although Midlarsky reported the upper and lower ranges only for the case of El Salvador, it is possible to infer those ranges from the degrees of freedom given in his Table 2, since the total number of data points used to compare the fit between the observed and theoretical distri-

Table 1. Midlarsky's Upper and Lower Ranges for the Log-Exponential and Exponential Analyses of Land Distribution in Latin America

			Upper Part		Lower Part	
Country	Year	Total Data Points[a]	Data Points	Range (hectares)	Data Points	Range (hectares)
Argentina	1960	9	5	200.1–10,000	—	—
Bolivia	1950	10	6	20.1–2,500	4	.1–20
Brazil	1960	4	5	—	—	—
Chile	1950	11	5	100–4,999.9	5	.1–49.9
Colombia	1960	16	5	50–2,500	6	.1–4.9
Costa Rica	1960	17	4	350–2,449.99	—	—
Cuba	1952	8	5	10–1,000	—	—
Dominican Republic	1960	10	5	4.7–1,572.5	—	—
Ecuador	1954	11	5	50–2,500	—	—
El Salvador	1960	13	5	50–2,499.99	5	.1–4.99
Guatemala	1950	11	4	447.2–8,943.99	—	—
Haiti	1970	18	5	5.2–25.8	—	—
Honduras	1960	10	5	35–1,749.99	—	—
Mexico	1960	9	7	10.1–5,000	2	.1–10
Nicaragua	1960	10	5	35–1,749.99	—	—
Panama	1960	9	4	50–1,000	—	—
Paraguay	1960	17	8	50–19,999.9	—	—
Peru	1961	15	5	50–2,500	4	.1–3
Uruguay	1960	11	5	200–9,999	—	—
Venezuela	1960	22	5	50–2,499.9	—	—

Source: Wilkie and Perkal 1985, 34–38.

[a] All categories -1, since the midpoint of the largest category cannot be estimated.

butions is two more than the degrees of freedom. The upper and lower ranges selected by Midlarsky for testing the exponential and log-exponential models in Latin American countries are listed in Table 1.[1]

There is extreme and inexplicable variation in Midlarsky's definition of what constitutes the upper range of large farms in each country. In the Dominican Republic large farms are those with 4.7 hectares of land or more. Large farms are defined as those with at least 20 hectares in Bolivia. At least 35 hectares are required for a landholding to be considered large in Honduras and Nicaragua. In Ecuador, El Salvador, Panama, Paraguay, Peru, and Venezuela the definition of a large farm is one with 50 hectares of land or more. In Argentina and Uruguay at least 200 hectares are required for a farm to be considered large. Large farms in Costa Rica are those with at least 350 hectares. And in Guatemala a large farm is defined as one with nearly 450 hectares or more. Most troubling about the apparently arbitrary selection of the upper ranges is that they contradict the basic land tenure patterns for some countries. For example, Argentina, with 457 million hectares of land in farms in 1960, is defined by Midlarsky as having a *lower* limit for his category of large farms than does Costa Rica, a country with only 2.7 million hectares of land in farms in 1963. Using the same source consulted by Midlarsky (Wilkie and Perkal 1985, 25), one finds 5.1 times as much arable land per capita for the agriculturally employed population in Argentina as in Costa Rica. More to the point, the average farm in the largest size cohort is 3.1 times larger in Argentina than Costa Rica. Yet, Midlarsky considers 200-hectares farms in Argentina to be large while Costa Rican farms need to be at least 350 hectares to be considered large.

Midlarsky determined by inspection that the lower range did not fit either the exponential or log-exponential distribution in most instances. Since the degrees of freedom were not reported for these cases, it is impossible—with one exception—to determine the boundaries of the lower range. The exception is Mexico, where seven of the nine data points were used to test the fit of the log-exponential model to the upper range. This means that the lower range must have comprised the two smallest categories, farms of up to 5 hectares and of 5.1–10 hectares. Otherwise, the lower range is up to 3 hectares in Peru, less than 5 hectares in Colombia and El Salvador, up to 20 hectares in Bolivia, and less than 50 hectares in Chile.

Since the operational definition of what constitutes the upper and lower ranges of land distribution is so variable across countries, it is impossible to compare them systematically. In some countries the upper range is restricted to extremely large farms of more than 200 hectares (Argentina, Costa Rica, Guatemala, and Uruguay), whereas in other countries it is expanded to include even farms as small as 5 hectares (the Dominican Republic). In some countries the total distribution is divided into only an upper and a lower part (Bolivia and Mexico), whereas there is apparently a middle part in the others. In one country (Chile) the lower range encompasses farms up to a size (50 hectares) that is included in the upper range in at least seven countries. Thus the fit of the observed to the theoretical distributions is essentially idiosyncratic, and there is no reliable way of grouping them into categories of patterned inequality.

Bifurcated Inequality

The data for our measure of bifurcated inequality are drawn principally from the 1950, 1960, and 1970 world censuses of agriculture conducted by the Food and Agriculture Organization (FAO) of the United Nations. The FAO (1981) reports information on landholdings for identical size categories of farms for each country: less than 1 hectare, 1 and under 2 hectares, 2 and under 5 hectares, 5 and under 10 hectares, 10 and under 20 hectares, 20 and under 50 hectares, 50 and under 100 hectares, 100 and under 200 hectares, 200 and under 500 hectares, 500 and under 1,000 hectares, and 1,000 hectares and over. We define a relatively small farm as one falling within the lower three levels of farm size—5 hectares or less. A middle-sized farm is defined as one falling within the middle four categories of farm size—5–99.9 hectares; and a relatively large farm is defined as one falling within the upper four levels of farm size—100 hectares or more. The size of the gap between large and small farms is measured by the ratio of the average number of hectares per large farm to the average number of hectares per small farm. This ratio of the average landholding of large farms to that of small farms—or land bifurcation ratio—is then weighted (multiplied) by the proportion of small farms in the country to yield a bifurcated inequality (BI) index (see the third, fourth, and fifth columns in Table 2). The weighting procedure is important theoretically because a bifurcated distribution of land will be more salient to the agricultural population, the greater the proportion of small farms.[2]

The index of bifurcated inequality is a quantitative variable that ranges from zero to a large number. If there are no large farms, the BI index will be zero. The BI index will be low if either the land bifurcation ratio is low or the proportion of

Table 2. Bifurcated Land Inequality in Latin American Countries

Country	Year	Hectares per Farm		Land Bifurcation Ratio	Proportion of Small Farms	Index of Bifurcated Inequality	Gini
		Small (< 5)	Large (≥ 100)				
Argentina	1960	2.8	895.0	319.6	.16	51.1	.86
Bolivia	1950	1.4	2,775.9	1,914.4	.59	1,129.5	.94
Brazil	1950	2.6	642.0	246.9	.22	54.3	.83
	1960	2.5	565.8	226.3	.31	70.2	.83
	1970	2.2	498.9	226.8	.37	83.9	.84
Chile	1955	1.4	1,188.1	848.6	.35	296.8	.93
	1965	1.7	1,232.7	725.1	.49	355.3	.94
Colombia	1954	1.8	419.2	232.9	.60	139.7	.85
	1960	1.6	419.6	262.3	.63	165.2	.86
	1971	1.3	418.1	321.6	.60	193.0	.86
Costa Rica	1950	2.2	551.0	250.4	.39	97.7	.81
	1963	2.1	463.6	220.8	.39	86.1	.79
	1973	1.7	360.9	212.3	.43	91.3	.81
Dominican Republic	1950	1.5	509.0	339.3	.76	257.9	.80
	1960	1.2	513.3	427.8	.86	367.9	.80
	1971	1.5	381.5	254.3	.77	195.8	.82
Ecuador	1954	1.7	536.5	315.6	.73	230.4	.86
	1974	1.6	317.4	198.4	.65	129.0	.81
El Salvador	1950	1.4	382.0	272.9	.80	218.3	.83
	1961	1.2	360.5	300.4	.85	255.4	.84
	1971	1.2	330.1	275.1	.74	203.6	.81
Guatemala	1950	1.7	245.1	144.2	.88	126.9	.87
	1964	1.8	354.8	197.1	.87	171.5	.82
Haiti	1971	1.1	0	0	.96	0	.48
Honduras	1952	2.3	351.8	153.0	.56	85.7	.75
	1966	2.5	262.5	105.0	.68	71.4	.75
	1974	1.9	331.1	174.3	.64	111.6	.78
Mexico	1950	1.4	1,621.5	1,158.2	.73	845.5	.96
	1960	1.5	1,439.2	959.5	.66	633.3	.95
	1970	1.7	1,331.8	783.4	.51	399.5	.93
Nicaragua	1952	3.0	256.9	85.6	.35	30.0	.76
	1963	2.6	245.6	94.5	.51	48.2	.80
Panama	1950	2.1	342.5	163.1	.53	86.4	.71
	1960	2.2	262.4	119.3	.46	54.9	.74
	1971	1.4	293.9	209.9	.47	98.7	.78
Paraguay	1956	2.2	2,940.0	1,336.4	.46	614.8	.95
Peru	1961	1.5	1,484.4	989.6	.81	801.6	.94
	1972	1.5	1,184.4	789.6	.76	600.1	.91

TABLE 2 (*continued*)

Country	Year	Hectares per Farm		Land Bifurcation Ratio	Proportion of Small Farms	Index of Bifurcated Inequality	Gini
		Small (< 5)	Large (≥ 100)				
Uruguay	1951	2.7	727.1	269.3	.14	37.7	—[a]
	1961	2.8	704.5	251.6	.15	37.7	.82
	1971	2.6	700.4	269.4	.13	35.0	.82
Venezuela	1950	2.1	1,531.1	729.1	.54	393.7	.94
	1961	2.3	1,160.4	504.5	.48	242.2	.92
	1971	2.1	941.7	448.4	.42	188.3	.91

[a] Insufficient data in the upper range for calculation of Gini.

small farms is low. The index of bifurcated inequality will be high if both the land bifurcation ratio and the proportion of small farms are high. Thus, where a process of bifurcation has created a polarized situation of many small farms and a few large farms, the index of bifurcated inequality will be high. At least one value for the index of bifurcated inequality can be calculated for every Latin American country except Cuba, and in 10 countries bifurcated inequality scores can be compared for the 1950s, the 1960s, and the 1970s. The relevant data are listed in Table 2,[3] along with the Gini concentration ratio.

In Midlarsky's (1988) view, El Salvador is the paradigmatic case of patterned inequality because it is supposed to have one of the most clearly bifurcated patterns of land distribution in Latin America. The average size of a landholding for small farms in El Salvador has been consistently one of the lowest in Latin America, initially 1.4 hectares in 1950, declining to 1.2 hectares during the 1960s and 1970s. However, the average size of a large farm in El Salvador has never been very great. Argentina, Bolivia, Brazil, Chile, Colombia, Mexico, Paraguay, Peru, Uruguay, and Venezuela all have a much greater average landholding among large farms than does El Salvador. Indeed, when the ratio of large to small farms is computed, the amount of land bifurcation turns out to be much greater in six other Latin American countries (Bolivia, Chile, Mexico, Paraguay, Peru, and Venezuela) than it is in El Salvador, which registers only intermediate values (see the Land Bifurcation Ratio column of Table 2).

Midlarsky contrasts El Salvador with Nicaragua, which is claimed to have a much lower level of bifurcation due to less land scarcity. This is an accurate observation. The average landholding of small farms in Nicaragua—initially 3 hectares in the 1950s, declining only to 2.6 hectares in the 1960s—was one of the three highest values in Latin America (small farms in Argentina and Uruguay being of similar size); while the average landholding of large farms in Nicaragua was among the three lowest values (Guatemala was similar and there were no farms of 100 hectares or more in Haiti). Consequently, Nicaragua had less land bifurcation than any other Latin American country except Haiti.

Costa Rica and Uruguay are presumed by Midlarsky to have the least amount of land bifurcation because in these countries the upper and lower parts of the distribution fit neither the exponential nor the log-exponential. However, as measured by the land bifurcation ratio, five Latin American countries (Guatemala, Haiti, Honduras, Nicaragua, and Panama) have had less land bifurcation than Costa Rica and Uruguay. Indeed, two of the three instances of very high patterned inequality (Colombia and El Salvador) have approximately the same land bifurcation ratio as the two instances of very low patterned inequality (Costa Rica and Uruguay). Thus, patterned inequality measures something other than land bifurcation.

Bifurcated Inequality and Political Violence in Latin America

Midlarsky (1988) argues that a bifurcated pattern of land inequality is a principal cause of political violence in Latin America. To test this hypothesis, he summed deaths from political violence (Taylor and Jodice 1983) over the years 1948–77, partitioned them into quintiles, and correlated them with the patterned inequality variable. Before computing the strength of association between these ordinal variables, Midlarsky adjusted the quintiles of deaths from political violence as follows: El Salvador was increased from the lowest quintile to the highest; Nicaragua was increased from the middle quintile to the highest; the Dominican Republic and Bolivia were reduced from the highest quintile to the second highest; Peru and Mexico were reduced from the second highest quintile to the middle; Haiti was reduced from the middle quintile to the second lowest; and Honduras was reduced from the second lowest quintile to the lowest. These changes were justified on the grounds that El Salvador and Nicaragua had experienced intense violence after 1977.

The "adjustments" in the level of political violence introduced by Midlarsky are subjective and arbitrary. They inflate the Tau_b correlation coefficient from .34 for the correct 1948–77 quintiles to .51 for his adjusted quintiles. Although the Tau_b value for the correct 1948–77 quintiles is significant at the .05 level, chi-squared (19.2 with 16 degrees of freedom) is not significant at even the .20 level. Since it is not obvious that patterned inequality is a legitimate ordinal variable, a conservative evaluation of the evidence would give greater credence to the lack of support for the hypothesis from the chi-squared test.

A second problem with Midlarsky's test of the inequality hypothesis entails his measurement of the dependent variable. Although deaths from domestic conflict events is a standard indicator of the magnitude or intensity of political violence in a country, gross death counts are hardly ever used because they are not comparable across countries differing in size of population. The usual procedure is to adjust directly for population by computing a death rate per some unit of population (typically one million population).[4] To construct a summary 1948–77 death rate variable we sum death counts over five-year intervals during that period, divide by midinterval population, and then compute the average. When the mean 1948–77 death rate from political violence is partitioned into quintiles, the Tau_b correlation between it and patterned inequality is .23, a value not significant at the .10 level (chi-squared is not significant at the .50 level).

The most appropriate way to test the hypothesis of a relationship between patterned inequality and political violence would be to dichotomize patterned inequality into the categories *absent* and *present* (or *nearly present*). This procedure at least avoids the level-of-measurement problem, although it does not solve the "apples and oranges" problem of inferring the presence or absence of patterned inequality from different definitions of the upper and lower parts of the land distribution.

The relationship between dichotomized patterned inequality and the 1948–77 political violence death rate is shown in Table 3. The point-biserial correlation (r for a quantitative and dichotomous variable) is quite small and not significant at the .10 level. The weakness of the relationship is illustrated by the fact that a majority of the instances of fits or near fits to patterned inequality—Peru, Chile, and El Salvador—had relatively low death rates from political violence.[5]

The patterned inequality variable is a seriously flawed indicator of bifurcation. Our index of bifurcated inequality is preferable because (1) it is a direct measure of the magnitude of bifurcation between large and small farms, (2) it is based on uniform definitions of the upper and lower ranges of land distribution, (3) it is a quantitative variable not subject to any ambiguity about level of measurement, and (4) it takes into account the proportion of small farms.

A plot of the relationship for Latin American countries between the mean index of bifurcated inequality during the 1948–77 period and the natural logarithm of the mean rate of death from political violence is shown in Figure 1. The death rate is logged in order to reduce the influence of extreme values. The correlation (r) between political violence and bifurcated inequality is .27, a value not significant at even the .10 level.[6] The reason for the weak association is that apart from Bolivia, all countries with very high levels of bifurcated inequality have had relatively low death rates from political violence during 1948–77 (Mexico, Paraguay, and Peru), while many countries with intermediate or relatively low levels of bifurcated inequality have had relatively high death rates from political violence (Argentina, Colombia, the Dominican Republic, Nicaragua, and Venezuela). Evidently there has been no long-term simultaneous association between bifurcated inequality and rates of political violence in Latin America.

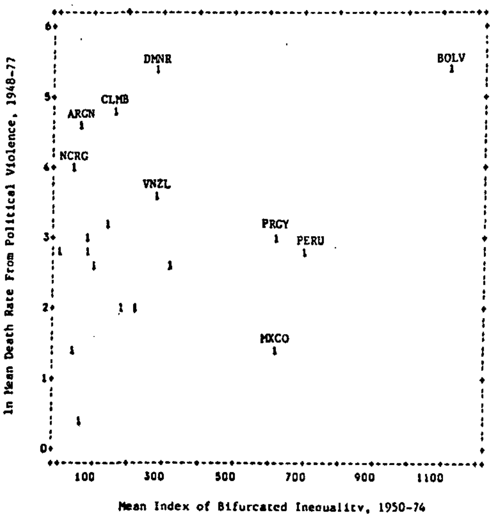
Another question is whether there is any lagged association between bifurcated inequality and political violence. To what extent would knowledge of a country's level of bifurcated inequality at one time predict its subsequent level of political

Table 3. Death Rate from Political Violence by Patterned Inequality: Latin America

Mean Death Rate from Political Violence, 1948–77	Patterned Inequality	
	Absent	Present
225.0		Bolivia
210.8	Dominican Republic	
116.5		Colombia
100.9	Cuba	
89.6	Argentina	
54.5	Nicaragua	
38.1	Venezuela	
21.4	Guatemala	
20.0	Paraguay	
17.2	Panama	
16.8	Honduras	Peru
15.3	Haiti	
12.8	Costa Rica	
12.6		Chile
6.7	Ecuador	
6.4		El Salvador
3.4	Uruguay	
3.2	Mexico	
.5	Brazil	

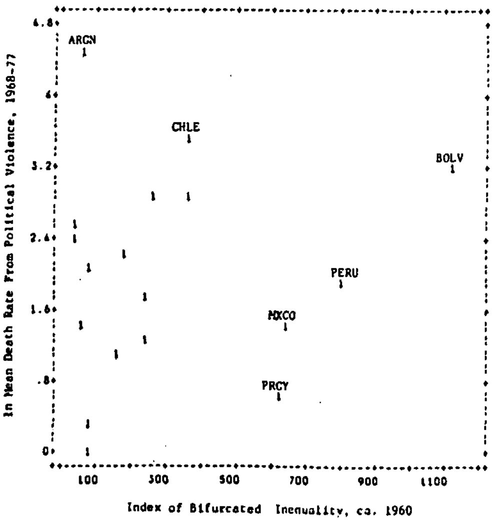
Note: $r = .23$.

Figure 1. Rate of Death from Political Violence by Bifurcated Inequality, 1948–77



violence? Although Midlarsky did not test the hypothesis of a lagged effect of bifurcated inequality on political violence, he stipulated in his theoretical discussion that "some time lag is required for the development of discontent by the peasantry" (1988, 496). Figure 2 shows a plot of the relationship between bifurcated inequality circa 1960 and the logged political violence death rate during 1968–77 for 18 Latin American countries.[7] The time-lagged correlation between political violence and bifurcated inequality is only .11. Argentina and Chile had relatively high rates of political violence during 1968–77 despite low-to-intermediate levels of bifurcated inequality circa 1960; Mexico, Paraguay, and Peru, again, had relatively low rates of political violence despite very high levels of bifurcated inequality; while Bolivia, with by far the highest level of bifurcated inequality, had only the third highest rate of political violence. Thus there is no predictive association between bifurcated inequality and political violence in Latin America.

Figure 2. Rate of Death from Political Violence by Bifurcated Inequality, 1968–77



Discussion

The search for a systematic relationship between patterns of land distribution and political violence has been long and frustrating. Supporters of the hypothesis of a linkage between the two will be disappointed by the results obtained in this analysis. While we have found no evidence to support Midlarsky's claim to have resurrected land distribution from the theoretical scrap heap, we do agree with his conclusion that "if one seeks to disclose relationships between inequality and political violence, the more sensitive procedure and one with stronger systematic variation with the dependent variable is to be preferred" (Midlarsky 1988, 504). This is why we prefer to rely on income inequality, which is not only associated significantly with political violence world-wide (Muller 1985; Muller and Seligson 1987) but also is correlated negatively and quite strongly with regime stability among democratic countries (Muller 1988).

Midlarsky has introduced a new measure of land distribution—patterned inequality—that in his opinion holds a major advantage over the Gini index.[8] We find his measure interesting conceptually but so flawed in construction as to be of little utility. We have proposed a more reliable and, in our opinion, more valid operational definition of bifurcation—a bifurcated inequality index, belonging to the family of measures of extremeness (see Alker and Russett 1966, 353–54)—which, however, was of no use in predicting violence.

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Edward Muller, Mitchell Seligson and Hung-der Fu have rendered a service to the debate over the origins of mass political violence. Their critique led me to discover two minor errors concerning citation of sources. Far more important, their basic misunderstanding led me to the discovery of a pictorial model of patterned inequality that I think goes far to explain both the origins of land distributions and the later appearance of mass political violence in regions with a relatively recent history of conquest and settlement, such as Latin America.

I will (1) directly address the issues raised by Muller and his colleagues; (2) briefly present the pictorial model of patterned inequality that has near-universal—or at least widespread—applicability, as shown not only by evidence presented in the original article but by its universal mathematical base; and (3) introduce arguments and systematic evidence to explain why the Gini index of land inequality has in the past demonstrated a marginal—although typically a significant—relationship with measures of political violence, why patterned inequality is superior, and why bifurcated inequality would not be expected to demonstrate such a relationship.

The Muller-Seligson-Fu Critique

Before proceeding to the heart of the Muller-Seligson-Fu critique and *their* emphasis (not my own) on bifurcated inequality, I should like to dispense with some of their procedural complaints. The first and most important of these is the variation in the upper range of large holdings across countries (Table 1). As was made plain in my article (p. 492), the theory of patterned inequality is based on a fair degree of context dependence. It is clear that given the different social and economic histories of countries and even of regions within a country, the pattern for the upper range will necessarily differ across countries. The differing geographic constraints of the various Latin American countries *must* eventuate in different cutoffs for the upper portion of the distribution and then, by extension, for the lower range as well. Mixing of the upper and lower patterns necessitated the omission of the middle portion in many instances.

Worse yet, Muller and his colleagues fundamentally misunderstand the relationship between the degrees of freedom and the cutoffs for the upper and lower ranges, despite clear indications in the article that such inferences cannot be made. They infer the cutoffs from the reported degrees of freedom for the chi-squared statistics despite the clear statement in footnote *a* to Table 2 (Midlarsky 1988, 498), "Where the expected value was below 1.5, adjacent categories were combined for the chi-square test until that figure was obtained." Such inferences cannot be made, because the degrees of freedom plus two, will actually correspond to the number of categories only where category combinations were unnecessary. Thus, whereas Muller, Seligson, and Fu infer that in the case of Costa Rica the upper range began at 350 hectares, in fact it was 199.5 hectares. In Ecuador and Panama, the upper range began at 20 hectares, not 50 as they inferred. And in Guatemala the land distribution pattern clearly dictated the log-exponential to begin at 1.4 hectares, not 450 as they inferred. For the sake of completeness, Table 4 includes the actual cutoffs for the upper and lower ranges. I excluded them from the original article on the grounds that six tables (plus appendix) were enough.

As can be seen in the fifth column, which gives the proportion of distribution area included in the upper range, it is not the cutoff per se that is important but the proportion of agricultural land area included in this range.[9] The values make in-

Table 4. Cutoffs; Data Sources; and Correlations between the Proportion of Distribution Area in the Upper Range, the Gini Index, and the Index of Bifurcated Inequality

Country	Year	Upper Range Cutoff (hectares)	Lower Range Cutoff (hectares)	Proportion of Distribution Area in the Upper Range ($r_{12} = .85$) (1)	Gini Index[a] ($r_{13} = .45$) (2)	Bifurcated Inequality[a] ($r_{23} = .61$) (3)
Argentina	1960	200.1[b]		89.58	.86	51.1
Bolivia	1950	20[c]	10[c]	99.37	.94	1,129.5
Brazil	1960	50[d]		86.99	.83	70.2
Chile	1950	100[b]	99.9[b]	92.60	.93	296.8
Colombia	1960	50[b]	4.9[b]	75.80	.86	165.2
Costa Rica	1960	199.5[b]		50.77	.79	86.1
Dominican Republic	1950	50.31[e]		53.33	.80	257.9
Ecuador	1954	20[b]		83.37	.86	230.4
El Salvador	1960	50[b]	4.99[b]	57.50	.84	255.4
Guatemala	1950	1.40[b]		96.69	.87	126.9
Haiti	1970	5.17[b]		19.91	.48	.0
Honduras	1960	35[b]		60.12	.75	71.4
Mexico	1960	10[b]		98.81	.95	633.3
Nicaragua	1960	35[b]		85.27	.80	48.2
Panama	1960	20[b]		77.52	.74	54.9
Paraguay	1960	5[b]		99.03[f]	.95	614.8
Peru	1961	50[g]	3[g]	86.21	.94	801.6
Uruguay	1960	200[b]		85.10	.82	37.7
Venezuela	1960	50[b]		92.01	.92	242.2

[a] After Muller, Seligson and Fu's critique, Table 2.

[b] Wilkie and Perkal 1985.

[c] Heath, Erasmus and Buechler 1969, 35.

[d] Roberts and Kohda 1967.

[e] Food and Agriculture Organization 1955.

[f] Area is for 1950, the only data for area given in the source.

[g] Food and Agriculture Organization 1966–70.

tuitive sense on examination. More important, they reveal strong systematic variation with the Gini index.

The criterion for choosing the cutoffs, it must be emphasized, was *not* a uniform numerical value across cases but the point in each instance at which the patterns of the log-exponential and exponential were observed to begin or end. This kind of uniformity is constant in pattern though different in number and is far more faithful to the uniqueness of the historical record than is Muller, Seligson, and Fu's choice of an arbitrary number to be applied across all cases. The issue, in other words, is not the size of a large or small farm (I never used that terminology) but the point of beginning or ending of a particular pattern. The use of the pattern allows for the assessment, by such interested observers as the peasantry, of change over time and direction of change.

It is claimed that I distorted the findings by including Nicaragua and El Salvador in the highest quintile of the 1977 data. (In this connection, I was pleased that Muller and his colleagues confirmed the distinction between the revolutionary patterns for Nicaragua and El Salvador found by Kenneth Roberts and myself.) I regard their concern about the dependent variable to be of secondary importance, because even with their "corrected" data the relationship between patterned inequality and deaths from political violence was statistically significant. Their attempt to discredit their own finding by attacking the ordinal basis of patterned inequality—clearly explicated theoretically in my original article—is arbitrary, to say the least. In any event, the inclusion of Nicaragua and El Salvador in the highest quintile is justified by the arguments developed in my note 7 (pp. 506–7).

During the 1948–77 period, the Salvadoran, and especially Nicaraguan, revolutions were ongoing, with the former just beginning to enter its most violent phase (Leiken 1984) and the latter already well underway (LaFeber 1983). Very likely there was a time lag in the reporting of events—especially deaths due to political violence—that led to their omission from the data source. Indeed they were omitted from the later updated Inter-university Consortium for Political and Social Research listing (until 1982) that I rejected for this and the other reasons indicated in note 7. In any event LaFeber (1983, 239), for one, is clear about the tens of thousands of deaths occurring between 1972 and 1979 that simply did not appear in the 1977 data compilation. A similar argument exists for the Salvadoran revolution. It actually found its beginnings in the formation of the Popular Forces of Liberation (FPL) in 1970 and the gradual insurrection that followed, including the mass killing of demonstrators in San Salvador in 1975 (Leiken 1984, 115–17). Omitting this information would have been tantamount to ignoring the obvious.

The issue of using absolute numbers instead of deaths per unit population is a matter of analytic preference and varies especially according to the theoretical orientation of the analyst. Using death rate as the dependent variable entails the hidden but important assumption of a proportional or linear relationship between the likelihood of deaths due to political violence and population size. This assumption is frequently unwarranted. But if one assumes, as I do, that potentially long-term revolutionary processes—not isolated periodic disturbances—are the likely outcome of patterned inequality and that such processes are driven by a frequently nonlinear dynamic, then population size is much less relevant and may even mask important relationships (as in Muller, Seligson, and Fu's Table 3). Contrast the large numbers of deaths due to political violence in El Salvador or Nicaragua with the much smaller number of deaths in Cuba despite the larger population size of the latter country. The near-genocidal behavior of the revolutionary Khmer Rouge in Cambodia, even against its own people, is another case in point.

The core of the critique, though, is the creation of a measure of bifurcation based on theoretical notions that they inaccurately attribute to me. Perhaps the strength of the original argument may be gauged by my critics' having to generate such an artifact, whose lack of validity was then easy to demonstrate. I never put forward a theory of bifurcated inequality but rather of *patterned inequality*, in which bifurcations were *always* associated with the presence or absence of a particular pattern. This was crucial to the argument, as I made clear throughout the article but especially in the concluding paragraph (p. 505). There the *predictability* (italicized in the original) of the peasantry's impoverishment relative to another, largely unchanging distribution at the upper levels was a key factor in the willingness of the peasantry to engage in political violence.

It was made explicit in the article (pp. 494–98) that these distributions were the consequences of long-term processes that had begun centuries earlier. As a keen observer and interpreter of the environment, the peasant is in a unique position to assess changes or their absence therein. I even included the Irish illustration in my note 9 to demonstrate the subdivision over time in agricultural landholdings at the lower (peasant) levels, in contrast to the absence of such change at the upper levels of the land distribution. This option was not available for the Latin American holdings because of the relative recency of those data. A dynamic aspect found throughout the original article is clearly absent from Muller, Seligson, and Fu's emphasis on bifurcated inequality. The patterns that are the core of my theory are collapsed into two numbers—the means at the upper and lower ends—thus reducing the process of change captured by these patterns into static entities. Not only is the original dynamic theory transformed beyond recognition by Muller and his colleagues, but large quantities of information are lost through this procedure. Distributions with a total of 10 data points at the upper and lower ranges are reduced to 2. This loss of information *in itself* could yield the nonsignificant results they obtain.

In a real sense the essentially static cross-national method of analysis, which has likely contributed much to obfuscate the relationship between inequality and political violence, is here resurrected in order to attack the far more dynamic and process-oriented theory of patterned inequality. I find their critique puzzling because in a fine recent article on democracy and inequality, Muller (1988) explicitly opted for a longitudinal approach to correct the earlier (mistaken in his view) finding of the absence of any relationship between the two variables using a cross-sectional approach.

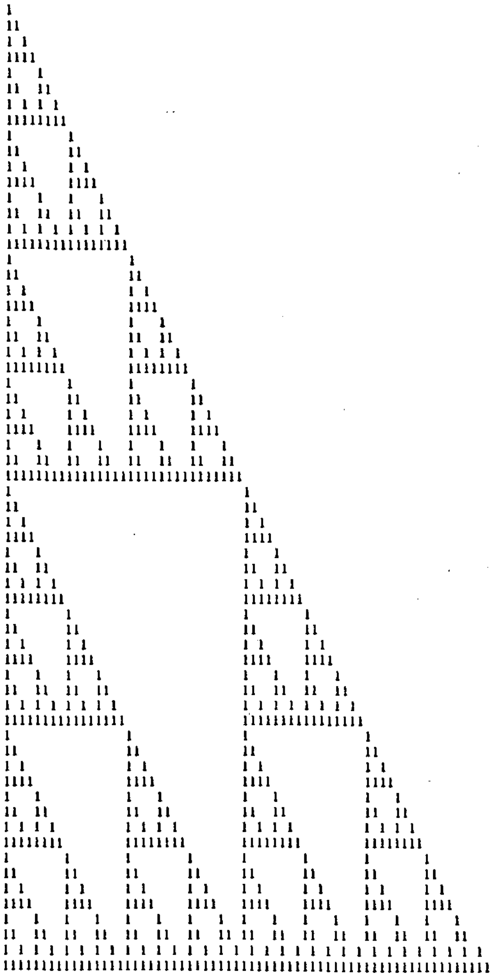
The Theory of Fractals

The adequacy of any theoretical framework is to be judged by a variety of considerations, not the least of which is the coherence and universality of its analytic core. And here the theory of patterned inequality rests on even stronger foundations than I had earlier suspected. The reason is that the overall pattern I had conjectured has a strong basis in the theory of fractals, which has only recently emerged as a branch of applied mathematics (Mandelbrot 1982). The Pareto-log-exponential distribution found to fit so many of the upper ranges of landholdings (Midlarsky 1988, 498) is a fractal distribution, as are many other natural and social phenomena. To my knowledge, this was the first identification of the upper range of land distributions as fractal, in tandem with the understanding of the upper ranges of income distribution in capitalist societies as Pareto-distributed (Lydall 1968) and therefore also fractal.

The essential characteristic of a fractal is self-similarity. As one reduces or increases the fractal pattern, it remains the same at all levels of observation. The pattern of a coastline and the cumulative winnings of a coin toss gambler with a fair coin are additional illustrations of this regularity (Feller 1968, 87; Mandelbrot 1982, 25–31, 240–41). Encroachments, such as the sea on the coastline or—more to the point here—conquests in Latin America after the Iberian invasions, illustrate this process. Indeed, there exists a model for conquest that predicts a fractal pattern in such instances of steady expansion. The model itself can be generated in an exceptionally versatile manner. All that is required is a specific rule set for steady expansion and a confined space over which the expansion will take place (Barnsley and Demko 1985). Even the random selection of outcomes as in the coin toss illustration, will after a time (say, 50 moves) yield a fractal pattern (Barnsley 1986). A variety of fractal patterns can be generated with very simple rule sets and in a random fashion. I will provide two illustrations, both of which are appropriate as models of conquest.

Consider a rule set for generating a particular configuration on R, a defined space (a plane in this instance, as on a computer screen) and for transitions from one state to the next on R. One example is given by a matrix consisting of a single one, and zeros that can be transformed to ones only under the certain conditions. The rule set G assigns a one if either there was a one in that cell at the previous move or there was a zero in that cell at the previous move but the neighborhood of that cell—consisting of all adjacent cells—now sums to one (mod 2).[10] In all other circumstances the cell entry remains a zero. At each move the cells are chosen randomly (say by a coin toss) as to direction of the move, then the cell entry is evaluated according to the preceding criteria. Eventually, after about 50 or so moves, a pattern begins to emerge not unlike that shown in Figure 3. Willson 1986 contains a more complete description of this procedure.

Figure 3. Fractal Pattern Emerging in the Pascal Triangle



Source: Chaitin 1987, 12.

The expanding ones can be viewed as *surrounding and occupying* choice territories (zeros, shown in the figure as blank spaces) for their own use as, say haciendas, while militarily *dominating* the remaining indigenous population centers (signified by the clusters of ones). Thus, there continue to exist in the clusters of ones militarily dominated local communities, and the conquerers make use of their tracts of land—large and frequently depopulated by disease—as they please.

The triangle below was generated by a different rule set (the Pascal triangle) that nevertheless obeys the general criteria, mentioned earlier, of steady expansion on a defined space. We begin with ones in the following positions:

1
1 1

Each entry will be the sum of the number directly above it (zero if none is explicitly there) and the number at its upper left. Thus, after several moves, one obtains

1
1 1
1 2 1
1 3 3 1
1 4 6 4 1

The pattern moves to the right and is not immediately obvious until all even numbers are treated as zeros and all odd numbers are ones, thus yielding Figure 3

(Chaitin 1987, 8–11). The zeros now occupy a set of triangles with a ratio of areas that is fractal, in this case Pareto-log-exponential. Zeros are removed from Figure 3, leaving blank spaces, to highlight the fractal pattern.

Note that the same or a similar pattern can be achieved either randomly, as in the first illustration, or by nonrandom methods, as in the second. In either case, a fractal pattern of triangles emerges with the precise ratio of areas—or, in this case, inequality relations—of the Pareto-log-exponential distribution. These are but two illustrations of the many (perhaps infinite) ways of yielding fractal patterns under very simple rule sets of expansion or conquest over a particular domain R.

Now the triangular pattern in Figure 3 is just as interesting for what it does *not* explicitly show as for what it does. In addition to the fractal pattern yielding a Pareto-log-exponential distribution, there exists the remainder of the area, indicated by ones, unoccupied although dominated militarily by the conquerer. This area is subdivided into triangles of equal area that would presumably be left to the indigenous peasantry after such a conquest. (It is immaterial whether one includes the isolated zeros in the "indigenous" triangles or opts for smaller triangles exclusive of the zeros; whichever option is chosen, all of the triangles have equal areas.) The peasantry would initially have equal land areas, which then, via the reproduction process and land subdivision, would yield the exponential distribution for the lower range of holdings. This process of subdividing initially equal holdings, thus yielding the exponential distribution, was conjectured earlier (Midlarsky 1982; Midlarsky 1988, 493–4; Midlarsky and Roberts 1985), but until the discovery of this large and rapidly growing body of applied mathematics it remained a conjecture with some empirical support but without direct analytic confirmation as shown here in Figure 3.

It must be emphasized that fractal patterns are the *typical* outcomes of random processes that obey rule sets along the lines of the two preceding illustrations (Barnsley 1986; Barnsley and Demko 1985; Willson 1986). Now it becomes clearer why so many of the distributions in the upper range of Latin American landholdings conformed to the log-exponential distribution (Midlarsky 1988, 498). These distributions resulted from conquest or expansion in one form or another, which, according to the fractal model, should yield the Pareto-log-exponential distribution. It is also clear why the cutoffs for the upper range *must* vary across the individual Latin American land distributions. Each of the fractal patterns emerges after expansion over a uniquely defined space, R, so the pattern that emerges is uniquely dependent on that space. Topography and any other environmental or social constraints affecting the direction of conquest also yield different fractal patterns that will inevitably result in different cutoffs for the patterns at the upper range. Cutoffs for the lower range also will vary now as the result of variations in the fractal pattern that determine the overall pattern of landholdings.

Understanding the Gini Index and Other Measures

It is interesting that despite the wide variation of upper-range cutoffs across countries—understood in the context of the theory of fractals—the proportions of land area resulting from these cutoffs show strong systematic variation with other measures, especially the Gini index. Table 4 gives Pearson correlation coefficients between the proportions of the total land area included in the upper range and the Gini values calculated by Muller and his colleagues in Table 2. This is an approximate analogue to the use of the upper 20% of the income range as a measure—really a surrogate measure—of inequality (e.g., Muller and Seligson 1987). The percentage of the variance held in common by the Gini index and proportion of land area included in the upper range is 72%, a rather substantial value. This finding, of course, empirically refutes one of the more serious claims of Muller and his colleagues, namely, that the variation in cutoffs for the upper range is without systematic foundation. Note that the Gini values are those given in Muller, Seligson, and Fu's critique. This condition stands in contrast to both the 37% of the variance held in common between their measure of bifurcation and the Gini index and the even-more-substantially-reduced 20% of the variance shared by the bifurcation index and the proportion of land area in the upper range (see Table 4).

These findings have several implications in addition to that of the systematic variation of the proportion of land area in the upper range. First, the Gini index is dominated by the upper range of holdings—as I suspected, given the vast difference in inequality between the log-exponential (upper) and exponential (lower) portions shown in the appendix (Midlarsky 1988, 505–6). Thus, the Gini index will tap the grosser sorts of inequalities inherent in the upper range, but largely ignore the pattern (mostly exponential) in the lower range under conditions of heavy land subdivision. But this is precisely the range of holdings that includes the peasant majority of the agricultural population that will be available for recruitment into revolutionary movements. The upper range of holdings, by definition, explicitly ignores this critical popular component. Given the correlations in Table 4, neither the Gini index nor the bifurcated inequality index would be sensitive to this popular sector, which is explicitly and separately taken into account by the patterned inequality measure. Apparently, the bifurcated inequality index is insensitive to all of the validation measures used both in this response and in the Muller, Seligson, and Fu critique. Patterned inequality, on the other hand, examines expected and observed patterns at both ends of the distribution range, thus accounting for inequalities across the broad spectrum of both landlord and peasant.

In Table 4, which includes cutoffs for the upper and lower ranges and data sources for each country, two corrections are incorporated. First, the data for the Dominican Republic are from FAO 1950, not FAO 1960 as reported in the original article. The earlier date was chosen as the earliest most complete data source (to allow time for the peasantry to become mobilized), and the source was recorded as 1960 by an oversight. Second, the words *or earlier* were mistakenly omitted from the reference to the UCLA Latin American series cited in Table 2 of the original article. This omission led to the erroneous impression by Muller and his colleagues in note 1 that the upper range for Brazil was somehow misspecified. The earlier source for Brazil in the UCLA series is cited in Table 4 here.

Finally, a comment is in order on the Muller, Seligson, and Fu's preference for analyzing income inequality instead of land inequality. I have never juxtaposed land and income inequality and opted for one over the other as a source of political violence. Income inequality, indeed, may be related to political violence in ways not yet explored. At this time this is a subject of some controversy (e.g., Weede 1986, 1987).

My preference for land inequality as an initial focus of investigation emerges from several considerations. First, land is generally a fixed resource in mostly agrarian societies that can lead to zero-sum games between landlords and peasants. Income, on the other hand, can be generated in expanding economies—typically industrial—to avoid the zero-sum aspect that likely is critical to the onset of political violence (e.g., Midlarsky and Roberts 1985). Second, as a consequence of the relative fixedness of land area, one can make the scarcity assumption that is necessary for the deriviation of both the exponential and log-exponential distributions. In contrast, the assumption of proportional growth and a consequent log-normality have been found to fit the lower and middle ranges of income distributions, thus necessitating an entirely separate analysis of income inequality in relation to political violence. Clearly, this additional research should be carried out. It will very likely clarify the important, but as yet poorly understood, role of income inequality as a progenitor of societal discontent.

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Notes

1. We are unable to determine the upper range for Brazil because Midlarsky stated the degrees of freedom as three—implying five data points—whereas in Wilkie and Perkal 1985 only four data points are available for estimation.

2. A comparison of Costa Rica and El Salvador illustrates the point. Both countries have similar land bifurcation ratios. But in Costa Rica, where many farms are middle-sized (between 5 and 99.9 ha.), small farms are a relatively small proportion of the total; whereas in El Salvador the distribution of land is such that most farms are either relatively large estates or small subsistence plots, and small farms make up approximately four-fifths of the total (twice that in Costa Rica). Therefore, to construct a valid operational definition of the concept of bifurcated inequality, i.e., a condition in which there are a few great landlords, many poor peasants, and few middle-sized "yeoman" farmers, it is necessary to adjust the land bifurcation ratio for differences in the extent to which agrarian society is polarized between lord and peasant.

3. All data are from Tables 15.1, 15.2, and 15.3 of FAO 1981 except for Haiti, where the data are from Tables 2.2, 2.3, and 3.2; and Argentina, Bolivia, Chile, Guatemala, Honduras (1966 only), Nicaragua, and Paraguay, where the data are from Tables 302 and 303 of Wilkie and Perkal 1985. Some of the distributions reported in Wilkie and Perkal cannot be compared exactly with the standard size categories of the FAO, so we have used the nearest approximation. In the cases of Guatemala, Honduras, and Nicaragua the upper limit of small farms is 6.9 ha.; in the case of Guatemala, the lower limit of large farms is 44.8 ha.; in the cases of Honduras and Nicaragua, the lower limit of large farms is 70 ha.

4. An alternative but less-desirable procedure is to include population as an independent variable in a regression equation. (For critiques of this approach see Linehan 1980, 190–93; Muller 1985, 51–52).

5. Note that El Salvador ranks in the lowest quintile of political violence even when the death rate is used in place of simple death counts.

6. If the rate of death from political violence is not logged, the correlation is .42, a value marginally significant (.10 level). The moderate strength of the association is entirely due to the extreme case of Bolivia, however. If Bolivia is excluded, the correlation drops to −.06.

7. The years range from 1954 to 1966 except for Bolivia, where the 1950 score is used as an estimate of bifurcated inequality circa 1960. Information on bifurcated inequality circa 1960 is not available for Haiti.

8. Curiously, Midlarsky (1988, 504) claims that the Gini concentration ratio "is simply a mathematical measure (the area between one curve and another), without theoretical content." By contrast, in Alker and Russett's (1966, 358–62) comprehensive review of measures of inequality, Gini is considered to be one of the few measures with "explicit theoretical norms."

9. In Table 4, the cutoff for the upper range signifies all of the area above the tabled value, and the cutoff for the lower range signifies all of the area below the tabled value. In the actual testing, as in Tables 1 and 4 of the original article, the highest land area category listed in the source was excluded because it was open-ended and its midpoint therefore could not be estimated. However, this area is included in the upper range *pattern* by implication, and therefore is included in the values of the fifth column for proportions of the distribution area in the upper range.

10. The term *mod 2* refers to a cardinal modulus that gives the remainder after division by 2. Thus 5 mod 2 is equal to 1. In the illustration given in the text, all summations are divided by 2 and the cell entries therefore are either 1 or 0, depending respectively on whether the summations are odd or even. See Messer and Marshall 1986 (pp. 55–56) for more details.

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Forthcoming in September

The following articles, research note, and controversies, have been tentatively scheduled for publication in the September 1989 issue:

Brian J. Cook and B. Dan Wood. "Principal-Agent Models of Political Control of Bureaucracy." A Controversy.

Robert S. Erikson, Gerald C. Wright, and John P. McIver. "Partisan Elections, Public Opinion, and State Policy."

Steven E. Finkle, Edward N. Muller, and Karl-Dieter Opp. "Personal Influence, Collective Rationality, and Mass Political Action."

Charles H. Franklin and Leane C. Kosaki. "The U.S. Supreme Court, Public Opinion, and Abortion."

Micheal W. Giles and Thomas D. Lancaster. "Political Transition, Social Development, and Legal Mobilization in Spain."

Richard Gunther. "Electoral Laws, Party Systems, and Elites in Spain."

Gary C. Jacobson. "Strategic Politicians and the Dynamics of House Elections, 1946–86."

Charles O. Jones. "Mistrust but Verify: Memoirs of the Reagan Era." A Review Essay.

Kenneth Koford. "Dimensions in Congressional Voting." A Research Note.

Paul E. Peterson and Mark Rom. "Federalism, Welfare Policy, and Residential Choices."

Paul J. Quirk, "Cooperative Resolution of Policy Conflict."

Philip G. Roeder. "Modernization and Participation in the Leninist Developmental Strategy."

Barry R. Weingast. "Floor Behavior in Congress: Committee Power under the Open Rule."

Donald Wittman. "Arms Control Verification and Other Games Involving Imperfect Detection."

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THE MEANING OF DEMOCRACY

The Battle of Democracy: Conflict, Consensus, and the Individual. By Keith Graham. Totowa, NJ: Barnes & Noble, 1986. 261p. $28.75.

Models of Democracy. By David Held. Stanford, CA: Stanford University Press, 1987. 321p. $35.00 cloth, $12.95 paper.

The Theory of Democracy Revisited. By Giovanni Sartori. 2 vols. Chatham, NJ: Chatham House, 1987. 542p. $40.00 cloth, $24.95 paper.

Thirty years ago, liberal democracy excited little passion among political philosophers. The theoretical foundations of modern democracy either seemed obviously true, and therefore uncontentious, or were assumed to be ideological manifestations of bourgeois dominance and therefore unworthy of serious theoretical consideration. The study of democracy, then, had largely become an "empirical" enterprise, in which Western political scientists explored the question of why the First World was free and stable, while the Second and Third Worlds were either despotic, chaotic, or both. On most accounts, democratic theory emerged as an abstract description of current Western (indeed, often peculiarly "American") practice. Many even heralded the arrival of a stable consensus and the end of ideology. There were, of course, important exceptions to this generalization. But it could not be said then, as it can be said today, that liberal democratic theory inspired much sustained philosophical reflection. The three books under review here illustrate— and aspire to guide the reader through—a dramatic revival of interest in the bases of liberal democratic thought that shows few signs of abating. This revival represents the convergence of several disparate trends.

The eruption of political radicalism in the 1960s, it has often been noted, soon made a mockery of the "end of ideology" debate. Along with political action came a polarized diagnosis of the apparent crisis of democracy, with disagreement centering mainly on the issue of equality. Some thought that excessive demands for equality and entitlement were "overloading" democratic systems, others that the inequities of Western capitalism were preventing the realization of true democracy and conspiring to blunt the transformative potential of democratic movements. In either case, democracy became a lively subject of theoretical debate. At the same time it regained its normative cachet. This transformation is perhaps nowhere better illustrated than in the work of Robert Dahl, one of the most interesting and innovative of the empirical theorists of democracy. In his *Preface to Democratic Theory* (1956) Dahl had implied that there was relatively open access to the endless bargaining that constituted decision making in polyarchal regimes. More recently, in *An Economic Theory of Democracy* (1985), he abandons this complacent vision to argue that the structured inequalities of corporate capitalism threaten the reality of self-rule.

If the focus of the 1960s and 1970s was on what one might call the "equality assumptions" of democracy, attention in the 1980s has been increasingly fixed on its "liberty assumptions." The reasons for this shift are not always clear but owe something to the increasing political success of conservative political parties and to the demise of political Marxism. The rising tide of conservatism has elevated the claims of classical liberals to new prominence and focused debate on the justification of economic freedoms and weaknesses of the welfare state. Moreover, a pervasive sense of drift and a perceived loss of individual and national purpose have led, in the United States, to new critiques of liberal democratic theory (and practice) as psychologically and morally impoverished and to a surge of religiously inspired criticism of secular society. Even as we witness this rise of a New Right, the appeal of orthodox Marxism continues to wane in Western and Eastern Europe. Many there are seeking a different standard for public debate, one that directly confronts the role of reason in democratic politics. A concern with the possibility of "communicative rationality," for example, has tended to direct new attention to themes traditionally associated with the liberal elements in democratic theory—individual freedoms, procedural justice, toleration of minority views, the neutral state, and the bases of politics by discussion.

A reconsideration of liberalism, then, has dominated the intellectual agenda in the 1980s precisely because such a reappraisal seems—for different reasons—urgently necessary to several groups: liberals and social democrats on the defensive against new attacks by cultural conservatives , as well as disillusioned Marxists looking for guidance in a "postmodern" world. The result is that liberal democracy is now seriously addressed by political theorists and philosophers in both the continental and analytic traditions.

Given a climate in which liberal insights appear to strike the imagination with fresh force, one welcomes the appearance of Giovanni Sartori's new book, *The Theory of Democracy Revisited*, an expanded and revised version of his *Democratic Theory* first published in English in 1965. Like the earlier book it provides an impassioned, erudite, wide-ranging defense of what Sartori now calls the "mainstream theory of democracy," that is, liberal representative democracy fortified with the accumulated wisdom of the twentieth century. There is a coherent vision here and a wealth of historical example and allusion. But the book is filled with digressions—not irrelevant but circuitous and distracting—and a kind of tendentious academic style that will discourage even a determined reader (and probably lose most undergraduates). More disappointing than these defects of presentation, however, is the fact that Sartori's call to arms in defense of liberal democracy appears to be directed against an already defeated enemy.

According to Sartori, the mainstream theory of democracy reflects the accumulated knowledge of two thousand years of political trial and error. Its central insight is that liberal *garantisme*—restraint of arbitrary state power through the rule of law—is the essence of civilization under modern social conditions. The difficulty lies in grafting the dynamic forces of equality and democracy onto this heritage. In very Tocquevillian fashion, Sartori sees these forces as the potential sources of a new despotism. Equality is dangerous because it generates its own demand. However, "beyond the point of equality of access, policies of equalization are largely policies of *redistribution* and ultimately of *dispossession*" (p. 352). The ideal of democratic self-rule (impossible in any literal sense) is potentially destructive because its advocacy leads to irresponsible "hyperidealism" (p. 80). Sartori's solution is a measured reliance on the market (which allegedly bypasses coercion in favor of spontaneous coordination) and a politics of electoral democracy in which citizens do not actually govern themselves but are governed by minorities whom they freely elect and who remain more or less responsive to an autonomous public opinion that expresses itself freely. Where the so-called elitist theorists of democracy (with whom Sartori has a strong affinity) went wrong was to allege that this view of democracy was merely "empirical." All theories of politics, he argues, have both descriptive and prescriptive elements. He then rightly points to the implicit normative dimensions of his own view: a conception of "vertical democracy," in which leaders are meritorious and citizens self-restrained.

What are the threats to the recovery of this mainstream theory? The tone of Sartori's book on this point is beleaguered. The very language of politics, on his account, has been debased almost beyond recognition—by, among others, participationists, Marxists, and "stipulative" philosophers.

The largest single revision in *Democratic Theory Revisited* is the discussion of the split between elitist and participatory strains of democratic theory—a split that largely postdates Sartori's earlier work. His main point against participation theorists is the important one that they inadequately conceptualize the shift from "microdemocracy" (or sites where participation is feasible) to "macrodemocracy" (the level of representative government) (p. 15). But this claim would be made more effectively if Sartori seriously confronted their arguments rather than dismissing them as foolish and utopian. Perhaps more to blame for the "dismemberment" (p. x) of mainstream theory, however, has been the unwitting collusion of Marx and Wittgenstein. It is in the joint influence of Marxism and analytical philosophy that Sartori finds the greatest pollution of political discourse. Marxists are charged with hijacking the term *democracy* and exploiting its normative appeal. Even worse, the "sociologico-Marxist rendering of thought" (p. 502) subsumes the theory of liberal democracy under the generic term *ideology*, thereby suggesting that its validity cannot be assessed by any objective standard. Linguistic philosophy is to be disparaged for unwittingly making this misuse of the language of politics more respectable. If Marxism sacrifices "ideas and ideals on the altar of ideology" (p. 503), the Oxford philosophers apparently sacrifice them on the altar of frivolous distinctions and "sheer unawareness" (p. 266).

There are two puzzling aspects of this portrait of democracy under siege. It is strange that so historical a thinker as Sartori should argue that the attempt to use words as weapons is more characteristic of contemporary politics than of any other. It is not intuitively obvious either that there was a relatively coherent discourse of politics up until World War II or that this discourse is now in shambles. But more important, Sartori seems to be combating foes that have melted away. It is simply not true that the market system today prompts more hostility than recognition of its merits (p. 410) or that the words weighing most heavily on the density of our time are *bourgeois*, *capitalist*, *reactionary*, and *fascist* (p. 482) or that philosophy contents itself with stipulating definitions. Market virtues are everywhere praised; the most frequently heard epithets in our political vocabulary are perhaps *permissive*, *interventionist*, *liberal*, and *totalitarian;* and analytical philosophy has entered the political lists with a vengeance.

It would be hard to find a more stark contrast to Sartori's perspective than that of Keith Graham's, *The Battle of Democracy: Conflict, Consensus, and the Individual.* Graham's book is a theoretical account (informed by recent British and U.S. philosophical discussions) of the grounds of democracy. It concludes with a plea to reconsider the claims of "strictly defined Marxism" to provide the most coherent expression of those grounds. It is a lucid and well-written study, and if its conclusions will strike some as highly dubious, the route to those conclusions is provocative and lively.

Deliberately invoking theorists like Rawls and Dworkin, Graham argues that the ideals of democracy cannot be adequately captured in an argument about interests but must imply a commitment to the rational autonomy of individuals. More strongly than they, however, he argues for an "integrated" view of liberty that does not acknowledge any coherent reasons for sharply distinguishing negative from positive liberty. Impediments to rational autonomy from any source (e.g., political coercion, economic circumstances, innate incompetence) can all render an individual unfree. Civil liberties, then, are special only because of their close connection with rational agency. This definition of liberty is one of the paths that leads Graham to Marx, who offers the most "theorized" (p. 171) account of the social and economic impediments to freedom.

Graham next argues that although a conception of rationally autonomous self-governing agents is the most coherent rendering of the democratic ideal, one cannot arrive at an adequate theory of democratic decision making if one grounds such a conception on purely individualist premises. In order to make sense of how individuals actually behave, we have to adopt the perspective of ontological collectivism, that is, to recognize that an individual is partly constituted by his or her identification with different groups and that what he or she does as a member of those groups can sometimes be adequately described only in terms revealing its contribution to collective action. At first this opening to collective analysis seems relatively modest. Graham argues that the recognition of the reality of collectives merely raises the question "with what size and nature of collective can I sensibly and successfully identify?" (p. 114). It would be natural to find here a reconsideration of pluralism or perhaps of certain variants of participatory theory. But instead Graham assumes that this recognition of the reality of collectives has made the argument for identification with a "collective of the whole" more plausible and particularly made a reconsideration of Marx obviously necessary. It is hard to see why this is the case.

Graham offers two reasons for the desirability of "one big democratic community," neither of them particularly compelling. First, he argues that identification with smaller groups might make consensus on a societal level more difficult, rather than easier. Second, he assumes that the main arguments against such an identification with the whole are merely practical. "Untutored common sense" (p. 120) objects that the size, complexity, and lack of integration in modern societies make it impossible for the people as a whole to rule directly. These practical objections appear to me to be formidable enough and to be unanswered by

Graham's counterclaims that technology has solved the problems of participation on a vast scale and that mutual interdependence and mass communications could provide the basis for a new collective identity. But the real difficulty is that Graham does not seriously consider a more fundamental objection: the possibility that attachment to a community of the whole is itself an unattractive ideal. His argument in favor of a consensual society seems to be heavily dependent on the insight that "where consensus exists, one of the most disagreeable and morally offensive features of human interaction—coercion—can be avoided" (p. 108). Yet there are surely other disagreeable and morally offensive features of human interaction, features that have been, and will always be, potential costs of "consensual" politics. These costs Graham does not weigh, but they are likely to be on the minds of those—in West or East—who are finding reasons to rethink the virtues of liberalism. What Graham does consider is the problematical status of *praxis* carried out in Marx's name, that is, the legacy of Leninism. There are no surprises here for any attentive reader of Marx and Lenin. (Indeed, Sartori makes the same point, namely, that Lenin significantly distorted Marx's thought in an authoritarian direction). However, the problem for many who remain sympathetic to Marx's critique of capitalism is not that Lenin was no democrat but that Marx's own conception of democratic politics was ambiguous and flawed.

Whatever their other merits, then, Sartori and Graham offer accounts of democratic theory whose contemporary relevance appears somewhat limited. Sartori is mired in an old debate, and Graham's arguments (especially given the realities of contemporary politics) seem to amount to wishful thinking. One of the virtues of David Held's *Models of Democracy* is that it can at least be said to offer the reader a more accurate canvassing of the current literature and a more realistic sense of the possibilities of political action.

Models of Democracy is also the one book under review that succeeds in its attempt to offer an introductory monograph suitable for students. Held offers a typology—indebted to C. B. MacPherson but more carefully worked out—that is sensible and useful. Moreover, the presentation is clear, the explication of arguments generally accurate, and the suggested lines of criticism reasonable. There are always costs involved in presenting a survey of a complex subject. In Held's case these drawbacks are more evident in the first half of the book than in the second. Chapters 1–4 provide a general overview of political thought from the Greeks to Marx, with an emphasis on how some notion of popular rule figured in the thought of the "greats" of political theory. *Models of Democracy* attempts to situate these thinkers historically, explicate their theories, introduce readers to at least some secondary literature, and rescue other theorists who seem more relevant to contemporary concerns (e.g., Wollstonecraft). This is probably too ambitious a project for a mere 139 pages of text, and the resulting narrative is often superficial.

The second half of the book, on the other hand, is more than a mere survey. Here Held's focus is sharpened by a concern to assess the desirability and possibility of liberal social democracy. He provides an intelligent guide through the thickets of competitive elitism, pluralism, corporatism, neopluralism, neo-Marxist theories of the state, and the respective merits of the "overload" versus "legitimation" crises of democracy. Of particular interest is the observation that neopluralists like Dahl and Lindblom and pluralist Marxists like Claus Offe have come to share a certain amount of common theoretical ground. Held concludes with a consideration of the current polarization between the New Right and the New Left, and with some final eclectic reflections on a theory of "democratic autonomy" that would speak to the most valid criticisms raised by both.

This theoretical position exists only in very rudimentary form. But at least some of the issues it raises—in particular the appropriate economic conditions of democracy, the proper conceptualization of the line between public and private, and the need for novel justifications of a protected sphere of privacy—are not only important but seem likely to dominate the debate over the theory of liberal democracy for some time.

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Rethinking Democracy. By Carol C. Gould. New York: Cambridge University Press, 1988. 363p. $39.50.

Few ideas in recent philosophy and political theory have had the staying power of Isaiah Berlin's distinction between positive and negative liberty. Two decades ago Gerald MacCallum received widespread attention and support for his claim that the negative freedom-positive freedom distinction is misleading on the grounds that freedom is a single concept with three distinct components (actors, goals, and possible limitations on the realization of goals). Yet the dispute between advocates of negative and positive freedom has remained very much alive. Richard Flathman recently argued for the analytical and political superiority of negative freedom and the liberal polity that it recommends. In *Rethinking Democracy,* Carol Gould offers an important and ambitious defense of equal positive freedom as the proper normative grounding for the primacy of democracy in social and political life.

Neither Gould nor others argue as if they were simply emphasizing one element in the concept of freedom. Clearly, there is more at stake. And the stakes in this intensified philosophical debate have become greater because there is growing uncertainty about whether settled liberal principles are by themselves a sufficient guide to a politics of freedom. As impressively shown by Gould, there is also a strong belief that a viable theory of positive freedom can be constructed incorporating the strengths and rejecting the basic deficiencies in liberal (negative) conceptions of freedom, while avoiding the authoritarian impulses that Berlin and others have argued necessarily follow in the wake of positive theories of freedom.

Gould acknowledges that a strength of negative liberty lies in its respect for the freedom of individuals as individuals, reflected in the insistence that political and civil liberties are essential if individuals are to be protected from outside interference. Yet she argues that liberal individualism is flawed because it cannot adequately account for the social character of certain of our choices: the formal freedom to choose offered by negative liberty is too narrowly individualistic. Perhaps more importantly, theories of negative freedom and liberal democracy are too little concerned with providing the resources that give individuals genuine opportunities to act on the basis of their choices. Liberal theories have focused on the institutions and political processes of government, ignoring the need to foster freedom and democracy in social, economic, and cultural institutions.

At the same time Gould is aware of the dangers traditionally identified with positive conceptions of freedom—most notably, that ideas such as self-realization or self-development have often led to organic conceptions of society whereby individual "development" means being forced to accept values that are imposed by more "enlightened" elites. She constructs a theory of democracy—grounded in freedom as self-development—that carefully charts a path between the isolated freedom of liberal individualism and the authoritarian dangers associated with holistic socialism. Thus the task she sets for herself is as important as it is difficult.

Her primary claim, following Gewirth, is that the fact of human agency leads to equal positive freedom as the basic moral principle of political life. Support for this principle means that we must be as concerned with providing the resources (enabling conditions) needed to develop and pursue individual purposes as with the negative rights (constraining conditions) that protect us from interference by others. Gould's normative argument offers a theory that requires democratic participation to reach beyond the realm of government into social, economic, and cultural institutions. In particular, she offers a normative grounding for workers' self-management within the context of firms—a grounding that is explicitly meant to extend the right of democratic participation to those who work in all social institutions. Thus, her conception of democracy has a firmer normative basis and a far wider range than most contemporary democratic theory.

Gould draws from an impressive array of contemporary philosophy and social theory as she carries out her project. She develops a more thorough conception of positive freedom as self-development; constructs a social ontology of individuals-in-relations as the basis for positive freedom and democracy, building on the Kantian ideal of autonomy and Habermas' notion of undistorted communication; expands important conceptions of distributive justice; extends ideas about human rights, especially, the right to property; revises claims about the formation of the democratic personality; assesses the potential for achieving democratic control of technology; and even offers new arguments about the obligation to promote individuals' self-development through support for human rights within the context of international relations.

This book makes an important contribution to our understanding of freedom and its implications for democracy within the context of contemporary social and political life. The important project of constructing a more adequate theory of freedom and democracy for the circumstances we face is far from complete, however. Gould's autonomous agents, empowered as they may be, are poorly connected to the social and historical context that informs their choices. Her conception of positive freedom as self-development is still too ambiguous, and I doubt her confidence as to the meaning of self-development and as to what resources are needed to provide equal positive freedom will be widely shared. Without greater clarity and persuasiveness on these issues and the moral and political principles that support them, the controversy about positive and negative freedom will continue unabated. Advocates of positive freedom will continue their quest for a politics that supports individuals' self-development, and liberal individualists will continue to construct theories of the limited state. But Gould's book offers a powerful criticism of, and alternative to, prevailing theories of liberal democracy grounded in negative liberty. Her aggressively democratic theory will challenge liberals to make more of freedom than noninterference and to extend democracy beyond the realm of government.

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Democracy and Participation in Athens. By R. K. Sinclair. Cambridge: Cambridge University Press, 1988. 253p. $42.50.

Perhaps in reaction to the uncritical romanticizing of Athenian democracy and to the "democratic excesses" of the 1960s, a literature has emerged that in the name of historical realism and accuracy aims to demythologize the vision of a participatory polity. Athens was less democratic than we think not only because of its subordination of women and its dependence on slaves, metics, and an empire but also because of the role wealth, social standing, and geography played in the unequal distribution of power among the male population who were full citizens. It is partly in response to this controversy that Sinclair has written a book on citizen participation in Athens from 450 to 322 *B.C.* He wants to determine the possibilities and realities, the means and extent, the cost and consequences, and the institutional forum and forums of such participation. Why was politics so central for Athenian citizens? What responsibilities and obligations did it entail? Who did and who did not participate, in what ways, for what ends, and in what degree? How much was political participation a matter of "compulsion," and how much was it based on the willing enthusiasm of those insistent on determining their collective lives?

Sinclair's focus is institutional, which for him means the lot and leadership, the council assembly, and juries. The lot was significant not only because its use presupposed that politics was a matter of general competence and shared wisdom rather than a matter of expertise but also because it limited the opportunities for the emergence of powerful state officials, mitigated what would have been intense political rivalries and, with its rotation in office and principle of collegiality, provided an experience of power and participation for every citizen. Political leadership was crucial because there was an "inherent" need in Athens for "individuals whose standing or expertise or experience or ability enabled them to play an active role and take initiative" (p. 37). Two of the eight chapters and part of a third are devoted to the "hazards" and "rewards" of leadership, to how and why individuals sought positions of power despite the stringent accountability demanded of them and occasional ruthlessness directed at them by the demos. Three chapters discuss the council assembly and juries. Sinclair emphasizes how the council's large membership's changing composition and range of functions gave extensive opportunities for, and placed considerable responsibility on, Athenian citizens to become familiar with the part in debates and the formulation of policy and reminds us that each year several thousand Athenian citizens were selected by lot as dicasts or heliasts. Given such institutions, as well as others such as the *graphe paranomon* (an indictment brought by any citizen to block a law passed by the *ekklesia* or *boule* on the grounds of unconstitutionality), Athens was, at least for its citizens, a participatory democracy.

But "at least for its citizens" is no small qualification since, according to Sinclair, those with full civic rights were a very select group of men, probably never more than 17% of the city's population. Moreover, it is likely that the *boule* in the fifth-century, like other offices of state, was not open to members of the lowest (thetic) class of citizens. And even when offices did open up, there were inequalities or impediments to participation, such as lack of leisure and resources, distances from the city and established patterns of deference. For example, since the *boule* met 260 days a year, limited wealth probably acted as a deterrent in serving at all and certainly to contemplating a second term. Despite these and other inequalities, "what was remarkable . . . was the extension of political rights and the direct participation of large numbers of citizens" (p. 200).

This is a lucid, balanced, solid, and judicious book. Sinclair raises significant questions and is a reliable guide through the current controversies surrounding their answers. Because the book is written for an audience that (in the jacket's words) includes "students of politics" and "anyone with an interest in the ancient Greeks," it is easy to overlook the learning and care with which evidence is marshaled and arguments are presented.

Given these and other merits, it is unfortunate that Sinclair has defined institutions and participation so narrowly, that his understanding of political issues is as untheoretical as it is, and that he seems unaware of the contemporary debate about democratic participation, in which classical Athens so often figures. Because of this narrowness, the book may be less directly helpful to students of politics and political theory interested in ancient Athens than are the different but overlapping works by Davies, Forrest, and Vernant.

Because of how he understands institutions and historical evidence, Sinclair is forced to rely on fourth-century orators which he (not uncritically) uses as evidence for fifth-century political practices. Thus, his emphasis on the role of leadership in Athenian democracy is drawn from fourth-century sources, as is his discussion of the severity with which the demos dealt with such leaders. The problem is compounded by the inevitable lack of continuity when one marks out a historical period between whose beginning and end so much happened: a devastating plague, military defeat, oligarchic coups, loss of empire. For instance, in Thucydides, the plague intervenes between the luminous vision of Pericles' funeral oration and the chastened vision of Athens and politics in his third speech. Something unrecoverable has been lost, which is why Pericles now calls for rededication on the grounds that public good enhances private good, whereas he had suggested that men participated in public life not because it enhanced their private life but because it was their life. If Thucydides (or his Pericles) is right, then there is a significant discontinuity in the grounds and aims of democratic participation.

Because the book is untheoretical, it is not fully alert to the significance of the questions it asks. For instance, Sinclair is concerned with issues of compulsion and power. Yet there is no discussion of ideology, culture, or social structure to provide categories to think about compulsion in other than Hobbesian terms. Nor is there any discussion of power that would enable us to understand Herodotus' assertion that the Athenians became powerful only when free. Even when Sinclair does refer to Thucydides or Plato and relies on Aristotle, there are serious omissions. We get little sense that love of power and fame might draw men to enthusiastic participation in common endeavors, though that is a central theme in Thucydides' *History*. Nor do we get more than a few lines about Plato's critique of democracy in the *Republic* or Aristotle's argument that politics is a moral activity valuable in itself.

A more theoretical approach to the subject and a greater reliance on Thucydides and Plato might have led Sinclair to consider how democratic culture informs the workings of institutions and to a broader idea of institutions themselves. Certainly religious festivals and tragedy were both constitutive institutions of Athenian democracy and modes of participating in it. One could argue that as an essential source of political wisdom and cultural equality, tragedy helped make claims for democratic competence plausible.

Sinclair has written an admirable book within the limits he chose. Yet those limits inhibit our appreciation of the importance of the questions he asks. Given the merits of what he has done, it is a disappointment that he did not do more.

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POLITICAL THEORY

Women and the Ideal Society: Plato's Republic and Modern Myths of Gender. By Natalie Harris Bluestone. Amherst, MA: University of Mass. Press, 1987. 238p. $25.00 cloth, $11.95 paper.

Women in Political Theory: From Ancient Misogyny to Contemporary Feminism. By Diana H. Coole. Boulder, CO: Lynne Rienner, 1988. 324p. $32.00 cloth, $14.95 paper.

Feminist theory is an avowedly antitraditional enterprise, committed to the reinterpretation of the past through the refractory prism of women's present consciousness. Both Bluestone and Coole present closely reasoned, heavily referenced, contributions to this reconstructive process, taking direct aim at "classic" renderings of women's lives in works of political theory. Their works are part of the ongoing attempt to—as Hannah Arendt put it—"think what we are doing" by learning how political theorists in the "malestream" have marginalized, coopted, or obliterated women's place in public discourse. To the extent that both these books also cite and critique a "femalestream" of feminist political theory, they are eloquent reinforcements of the contemporary antitradition in the discipline, which is now well established.

Bluestone's approach is foundational. She argues that Western philosophy in general—and utopian thought specifically—derives from Plato's speculative polis. Plato raised, definitively, "all the fundamental questions of philosophy" and "all the major contemporary issues of sexual equality are touched on in the arguments of the Republic, Book V" (p. 167). In fact, "even the order in which Plato raised the questions still remains cogent." Bluestone presents a meticulous review of every recorded commentary on book 5 from the early nineteenth-century onwards, drawn from philosophy, classics, and theology as well as the tradition of political thought. Commentators on Plato are evaluated on the basis of their interpretation of each of Plato's "three waves," which are arrayed in descending order of significance beginning with the proposition that "there is no practice of a city's governors that belongs to woman because she's woman, or to man because he's man." Bluestone argues that since Plato's Socrates excludes considerations of biological function and physical strength from his evaluation of the souls of guardians, he is to be understood as endowing women with the qualities of rationality and leadership. The second wave, abolishing private property and the family, is simply the necessary deduction from the first, rather than the reverse. The argument of Okin, Annas, and others that Plato's abolition of private property in wives was the genesis of his reluctant inclusion of women among the guardians is an incorrect modernist reconstruction of Plato's intent, according to Bluestone (pp. 102–8). For her, a literal reading of the terms and sequence of a much-disputed section of the *Republic* is the evaluative standard for both authentic feminism and objective political theory.

Plato's nasty asides on the "dark" and chaotic natures of females in the context of private life are undeniable, agrees Bluestone, but were common among male Athenians. Plato's "rational" model transcends his own culture-based limitations. The important question first raised by Plato—and only intermittently addressed by his successors in the tradition—Bluestone argues, is whether it is in the interest of society that more than half its citizens' capacity for reason and leadership be underutilized. Her account of interpretive errors in Platonic scholarship reflects a vantage point she terms "moral utilitarian" or "functionalism." Plato becomes an equal opportunity theorist in a democratic age.

It is not possible to reconcile the radical with the "malestream" Plato, argues Bluestone. Nor is it useful to pause too long over Rousseau's jibe that "having dispensed with the individual family in his system of government, and not knowing any longer what to do with women, he finds himself forced to turn them into men" (p. 103). Androgyny, elitism, and the devaluation of plural, private selves are criticisms of Plato that have been raised by Annas, Elshtain, and Okin. But they are dismissed by Bluestone as misdirected feminist challenges to the importance of rationality in the conduct of human affairs. Reason and leadership constitute the contested terrain in the battle between feminist and patriarchal scholarship, Bluestone insists, and should not be conceded to the enemy in affective theories of women's "special" and particular nature.

Coole's work is antifoundational, as well as antitraditonal. It is a history of the tradition of political thought, incorporating unchronicled women theorists who were contemporaries of the male "greats" but also surveying contemporary critiques of the tradition by feminist scholars. Western thought, argues Coole, is fissured by a series of related "binary oppositions" rooted in sexual opposition, for example, public-private, universal-particular, reason-passion, domination-subjection. From Plato through modern Marxism varieties of feminism, Coole tracks the divided discourse of political theory on the assumption that polarizers are "conservatives" who seek to legitimize the ascendancy of the male principle. These include Aristotle, Aquinas, Rousseau, and Hegel. Women's place is established by biological determinism and cultural tradition in a natural hierarchy of function. The other half of the tradition, as Coole analyzes it, is an incipient antitradition, which, however, never escapes the language and assumptions of its twin. It is "radical," and includes Socrates, Plato, Augustine, Wollstonecraft, the Utilitarians, Marx and Engels, Beauvoir, and Firestone. But the radical effort to accord an equal status to women's intellect and productivity, warns Coole, is less a solution than it seems because "emancipated women are . . . those who approximate the male norm" of rationality and economic production (p. 3).

She is particularly interested in the difficult interface between traditional though "radical" liberal democratic theory and Marxism on the one hand and feminism of the antitradition on the other hand, which attempts to incorporate but reconfigure rights- or labor-oriented views of public life. Denouncing the entire tradition as "paternalism" does not necessarily generate the tools required to build a new antitradition whose norms and idioms of discourse are not bound by the assumptions of its paternity. In their historical contexts, Locke and Marx's view of gender-neutral labor, though hedged-in with contextual qualifiers, posed a liberating alternative for women. Productivity and democracy might be self-enhancing rather than exploitative under conditions of "pure" capitalism or communism. Like Bluestone's "first wave," however, the invitation is fraught with sacrifice. The price of liberation is the marginalizing of the personal sphere and with it the role of species nurturing and individuality, which is the foundation of the public world.

Coole's excellent and exhaustive thinking about what the tradition is doing to women returns, like Bluestone's, to a frustration at the limits of all patriarchalism, even its possibly radical variants. Coole acknowledges the radical implications of Plato's book 5. But she is convinced that "underlying these principles is a belief that women are in fact less capable of virtue, less rational and less ready to learn than their male counterparts" (p. 35). For both Coole and Bluestone, a return to the classics is still a mandatory pilgrimage of purification, a traumatic rite of passage to a mature feminist antitradition in political thought.

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The Human Element: Three Essays in Political Psychology. By A. F. Davies. Fitzroy & Ringwood. Victoria, Australia: McPhee Gribble/Penguin, 1988. 124p. $12.95 Australia.

Alan Fraser Davies of Melbourne University (1924–87) was a political psychologist whose scholarship, although informed by uncommon intelligence, sensitivity, and erudition, did not travel well. His work is insufficiently known outside of Australia for reasons intrinsic to its contribution. It is steeped in the most influential, but also the most controversial, twentieth-century psychological tradition—psychoanalysis. Its referents often are the specifics of the Australian political experience, so that what his psychoanalytic insights illuminate is occasionally unclear to readers from other countries. His writing is lapidary but so understated and allusive that his most important assertions are sometimes passed over by readers accustomed to prose that trumpets its conclusions. A valuable, brief review of Davies's work by his student James Walter appears in the "In Memoriam" section of the winter 1988 *PS* (p. 111).

The most important of the three posthumous essays in this volume—and the best as an introduction to Davies's scholarship—is "Guilt in Politics," based on a paper Davies gave in July 1987, a month before his death, at the annual meeting of the International Society for Political Psychology (ISPP). As he observed at the ISPP meeting, his essay on guilt is, in effect, an addendum to his 1980 book on political affect *(Skills, Outlooks, and Passions)*, which neglected to include guilt in its thesaurus like account of families of political emotions: moral indignation; anger and hatred; envy, inferiority, and resentment; fear, suspicion, and paranoia; powerlessness, disillusion and cynicism; sympathy and pity; and loyalty, pride, and trust.

"At the centre of the feeling of guilt is self-disappointment," Davies writes. "Guilt looks back, measures and condemns—though sometimes no further back than to deplore an intention or plan just conceived, some newly-framed image or idea. So we may also be thought to steer to a degree by it, scanning our probable actions in advance for its threat to them, trying not to repeat what has already caused us remorse."

Carefully distinguishing guilt from such related affective states as fear of punishment, shame, and depression, Davies elucidates the complexities of assessing an emotion that is often unconscious or, when it surfaces, is commonly defended against by repression, denial, or projection. He goes on to provide a valuable distillation of what psychoanalysts from Freud to Kohut have said about the origins and dynamics of guilt, concluding with a brilliantly eclectic array of illustrations of the role of guilt in politics: the British political psychologist Roger-Money Kyrle's typology of political guilt, *Psychoanalysis and Politics* (1951); Alexander and Margarete Mitscherlich's claims about the influence of unacknowledged guilt feelings on postwar German politics in *The Inability to Mourn* (1967); and E. R. Dodds's effort in *The Greeks and the Irrational* (1951) to show how the shame culture of Homeric times was transformed to the guilt culture of the fifth-century *B.C.* era of the great Greek tragedies. Davies adds to this literature review his own analysis of how guilt and confessed culpability manifested themselves in the 1985 episode in which the French government was discovered to have been behind the frogmen who blew up the *Rainbow Warrior*, a ship that was about to sail from New Zealand to protest France's nuclear testing in the Pacific.

In another of his essays, "The Politics of Being Central," Davies analyzes an unstudied "social wish" that he argues is far more ubiquitous than the much-discussed will to power—"the need to be central." Davies explores how one might seek to conceptualize and identify such a need—if it does exist—and shows how his distinctions illuminate a debate that took place in the 1960s about who was and was not central in a putative Australian establishment. Needs bearing on one's sense of centrality, Davies argues, are more likely to have "a deep root in . . . much of what we call character. . . . Set adults down in a strange situation and some will strive at once to join the central circle . . . ; others will vow to keep silent, anonymous, observing and judging only from safe cover . . . but most will feel contradictory urges, depending on what is at issue or being proposed and on the qualities and values the active protagonists display."

The essay that opens the collection, "The Demand for Political Psychology"—pieced together by his colleague Graham Little from the notes of Davies's 1987 seminars on political psychology—opens with a question of intellectual history. "When," Davies asks, "was the first occasion on which a serious political writer felt the need for a psychological explanation of a large political event, declaring it so strange, bizarre, mad, so impossible to account for in common-sense terms, that some quite special story about deeply hidden forces, motives or desires was required?" Davies's answer is that it was the Canadian-born economist John A. Hobson's 1900 effort in *The Psy-*

chology of Jingoism to explain the nationalist frenzy of the Boer War. His answer, however, is less important than the opportunity his topic gives him to introduce his students (and now his readers) to an intriguingly diverse sampler of psychoanalytically inspired explanations of seemingly inscrutable political behavior by, among others, Harold Lasswell, Karen Horney, Lucian Pye, and Robert Tucker.

Davies's work provides a literate, informed complement to the cognitive emphasis in the last decade's political psychology literature. Scholars who seek to understand political thought and motivation would do well to posit that the brain of *homo politicus* contains two lobes and avail themselves of A. F. Davies's explorations of political psychodynamics. (The book is available postpaid from the publishers, 66 Cecil St., Fitzroy, Victoria 3065, Australia for $12.95 Australian [about $11.14 U.S.].

FRED I. GREENSTEIN

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Max Weber: Essays in Reconstruction. By Wilhelm Hennis. London: Allen & Unwin, 1988. 197p. $39.95.

It remains a curiosity of modern social science why a figure of such extraordinary importance as Max Weber lacks a "school" or even an "orthodoxy" with regard to interpretation. This issue provides the underlying theme for the five essays collected in the Hennis volume. Hennis is interested in "reconstructing" Weber's worldview in order to move Weber scholarship from the simple repetition of fragments of Weber's thought to the creation of a general Weberian perspective. Such an ambitious project is likely to be both interesting and controversial. On this account, Hennis surely does not disappoint the reader.

The first two essays attempt to extract a "central question" or "theme" in the diversity of Weber's writings. In his first essay Hennis concludes that Weber was primarily concerned with the "fate of humanity" *(Menschentum)* in a changing social environment. Hennis supports this with Weber's 1910 work "Anticritical Last Word," written against Felix Rachfahl. This conclusion is clearly defensible. However, it is a statement of such a general nature that it could easily be accepted by those whom Hennis wants to confront, the defenders of the "rationality thesis" of Tenbruck and Schluchter. However, Hennis fails to show that a necessary conflict exists. On the contrary, Weber's concern for humanity would lead him to study the process that he saw undermining it. Therefore, the "humanity thesis" does not really convey much beyond what is self-evident in both the *Wissenschaftslehre* and *Protestant Ethic.*

The second essay, on Max Weber's "theme," provides a more important discussion. Hennis gives examples from Weber's writings of a subtle yet continual juxtaposition of the demands of the social environment with the "orders of life." It is in this assertion that we see both the triumph and the shortcomings of Hennis's analysis. How the requirements of capitalism have altered the necessities of social life is well elaborated, but Hennis makes only half of a very compelling argument. The reader is left lacking a clear elaboration of the natural components of "life" through Weber's eyes. In order for Weber to have held that modern social relations have had a negative impact on part of the human personality, Weber must have had some notion of what the "true" human personality is.

What is Weber's "ideal type" of the human being to which he compares mankind in its current social context? This is a piece of the puzzle that must be put into place if, as Hennis seems to desire, Weber is also to be adopted by the tradition of political theory. The failure to elaborate this aspect of Weber and, I suspect, the desire not to move too close to the "rationality thesis" causes Hennis to move Weber far too close to Marx. This problem is particularly acute in the linkage between capitalism and the rationalization of society. Hennis asserts that Weber attributed the rational depersonalization of social life to the growth of capitalism (p. 99). This has an air of monocausality that does not do justice to Weber's notion of "elective affinities." Here the idea that capitalism is part—albeit a major part—of a general process of rationalization would seem more consistent with Weber's methodological perspective.

Despite this methodological criticism, Hennis provides some valuable insights into how Weber saw the logic of capitalism impinging on the ethical components of life. Capitalism introduces a set of ethically neutral forces—the market—into the realm of social interaction

(p. 102). Here lies the contrast between pre- and postrational phases in social development. In rational society, social relations are no longer personal. They are subject to systemic (this is not Weber's terminology) forces outside of what are normally thought of as the parameters of ethical judgments. This insight could be very useful in the construction of a Weberian model for social critique.

The third essay, on the influences of the historical school, is the most problematic of the five. Its entire thesis is built on the notion that Weber either misunderstood the ideas of the historical school or, as a reflection of some sort of academic "patricide" (p. 141), purposely misrepresented the teachers from whom he was to adopt most of his economic perspective and methodology. Here, as with his discussion of Marx in the second essay, Hennis points out similarities, rather than the important differences, between Weber and his mentors.

Hennis makes a provocative argument for his case, showing parallels between Knies and Weber on the impact of historical conditions on the inner world of the individual; the similarity of their concern for economics as a community enterprise; and Weber's shared opinion with the historical school that economic competition lacks the benign character asserted by the liberal tradition. These are valuable insights into Weber's world view, but Hennis is unconvincing when he argues that Weber could not come to these conclusions within the framework of another methodological perspective.

Hennis's desire to downplay the neo-Kantian influence on Weber causes a dilemma. The only way to account for Weber's distinction of facts and values and inner and outer world experiences, his focus on the impact of rationality, *and* his criticism of the historical school is to put him back within the framework of a neo-Kantian epistomology. Hennis seems to admit that his own argument may be overstated when he writes that much of Weber's methodology stems from the neo-Kantian debate (p. 142). But if that is the case, where has Hennis really taken us in this essay?

Hennis begins his fourth essay by arguing that the extent of Nietzsche's influence on Weber had been, in the period after World War II, systematically understated in order to make Weber more acceptable to Western readers. Hennis then—often by indirect inference—links ideas of Weber to their origins in the writings of Nietzsche.

Hennis is hardly breaking new ground. Nietzsche's influence is discussed by Mommsen, Eden, and others; and unfortunately, Hennis fails to go beyond the perpetuation of a common problem. Weber's statement that "the honesty of a scholar is measured in his attitude toward Nietzsche and Marx" is often taken too literally. Nietzsche is a symbolic referent for a whole romantic tradition in German intellectual history that preceded him. Nietzsche was not the first to look at tragedy in human history, to see life as struggle, or to fear for the loss of the ethical and emotional side of human nature to the nihilistic petrification of externalized reason. These ideas can all be found in Schiller, Herder, Goethe, and the *Sturm und Drang*, with which Weber was very familiar. It is therefore likely that Weber's view of humanity would not have been too terribly different without Nietzsche.

In the final and most interesting essay Hennis examines the nature of Weber's "liberalism." Hennis proceeds by constructing an "ideal type" of nine characteristics associated with liberalism and then compares Weber's views to them. Building on his previous essays, Hennis links Weber's somewhat ecclectic political views to the ontological tension found in his *Weltanschauung*. Weber's skepticism about rationality, progress, and universal equality make him quite different from the classical liberal. There is simply too much romanticism in Weber to adopt such unbridled faith in reason. Reason alone will not drive humanity to its destiny.

Here Hennis provides a very useful insight into Weber's political motives—assuming that democracy for its own sake was not Weber's cause. At this point Weber's elitism and his defense of democracy merge. Democracy provides the best—and perhaps the last—opportunity for the "true politician" to emerge. It is in democracy that the type of person with the "will" capable of resisting the "iron cage" has the best chance of capturing the hearts (if not the minds) of the population.

Hennis embarks on his reconstruction in the only way possible: inductively. The work as a whole, therefore, opens a Pandora's box with regard to interpretation. Weber did not write specifically on human nature, his personal value system, or the ideal state. Therefore, scholars must inductively infer Weber's position from the historical record, personal accounts, and scholarly writings. Here, Hennis does have some problems. His conclusions are overstated, often with such certainty that the unsuspecting reader might well wonder how generations of scholars could be so incompetent to have missed the obvious. However, inductive inference will always have its inaccuracies.

In the light of the necessity for such inductive methods, is a Weber orthodoxy ever likely to develop? The historical record, as well as the wealth of writings, is always open to creative interpretation. Hennis seems to think that the problems will be solved with the completion of Weber's collected works. Then, in reality, the real problems of interpretation are likely to begin. As Weber taught us so well, the infinite complexity of any event can never be reconstructed conceptually. Therefore, the interpretive enterprise is likely to continue undiminished into the future as long as scholars continue to find something of relevance in this German social thinker. Unfortunately, this means that the establishment of anything close to a Weber orthodoxy is likely to remain an elusive goal.

However, placed within a somewhat less ambitious framework, Hennis has provided a unique and interesting interpretation of his own. He has also shown the importance of understanding Weber as a complete personality. Only with some perspective on the work as a whole will Weber's readers be able to understand not only *what* he said, but also *why* he said it. This cannot help but assist in moving the treatment of Weber from interpretation to application. This proves to be Hennis's greatest contribution.

ANDREW M. KOCH

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Ritual, Politics, and Power. By David I. Kertzer. New Haven: Yale University Press, 1988. 235p. $22.50.

In this book, David Kertzer, an anthropologist, tries "to show why ritual is important in all political systems" and argues "against the common view that political ritual merely serves to bolster the status quo" (p. 2). His theme is that politics is expressed through symbolism and ritual, where ritual is defined as "symbolic behavior that is socially standardized and repetitive" (p. 9). A subtheme is provided by his repeated reference to the sacralization of secular regimes.

The evidence marshaled throughout this book is in the form of tales and vignettes of political rituals from many times and places. As an illustration, in the second chapter alone one reads about a 1946 Ku Klux Klan meeting, the Soviet Communist party's issuing of new membership cards in 1973, Louis XIV, French Montagnards of the 1850s, the clan system of the Tallensi of West Africa, New Guinea male initiation rites, the creation of a new calendar by the first grand astrologer of the T'ang dynasty, the Monhegan Indians of New England and their efforts to renew tribal identity, Charles IX's two-year royal procession, black power rallies and chants, the simultaneous 1986 inauguration of Ferdinand Marcos and Corazon Aquino, and Nixon's use of "Hail to the Chief." The cases themselves make interesting reading and are used to illustrate familiar political themes, such as the symbolic representation and distinctiveness of organizations, the relating of local organizational activity to the national level, and the investiture of power or legitimation.

Two related issues about ritual are addressed. The first issue is why ritual and symbolic displays seem to be so necessary. Explanatory excursions are made into cognitive psychology (chap. 5) and ethology (chap. 7). Neither perceptual schema nor stylized conflict displays seem to provide satisfactory answers; and the concluding chapter, using what appears to be a vaguely Marxist or perhaps a vague elite theory orientation, argues that while there is symbolic diversity, there is also conflict over the "dominant symbolic paradigm, the struggle for hegemony," a struggle of "the privileged to protect their positions by fostering a particular view of people's self-interest" (p. 175).

The second issue, related to this last point, is the use of ritual by those seeking power. Kertzer amends Durkheim's proposition that ritual creates a solidarity of shared beliefs among all members of the society by noting that solidarity can also be produced in the absence of a commonality of belief. Thus, ritual is used not only by kings and presidents but was also employed by the Italian Red Brigades when they kidnapped Aldo Moro and by the Sikhs in their conflict with the Indian government. In "Rituals of Revolution" (chap. 8) Kertzer takes us through the manipulation of rites in the French, U.S., and Iranian revolutions—going to some length to point out how ritual can be used to overturn the political order—and for good measure adds a section on the Nazi use of symbols.

For political scientists, this book is an interesting addition to the literature on culture and politics. If one can make the distinction, this is a book about the culture of politics rather than political culture. Kertzer's intellectual debt is in any case more to Geertz and Turner than to Almond, Edelman, Lowi, or Wildvasky, not to mention Lasswell, MacIver, or Hobsbawn and Ranger.

One major drawback of this book is its imprecise theoretical orientation. Symbols are defined variously as a communicative device and the content of ritual and are supposed to have the properties of condensation of meaning, multivocality, and ambiguity. Reference to semiotics may have been intended, but Kertzer fails to formulate his usage of the term clearly. Ritual, by contrast, is clearly defined, but the sweep is so broad that any example from any culture is apparently appropriate, as the above list from the second chapter indicates. There is a remarkable sentence near the end of the book, which begins, "As is evident in the cases of Aztec cannibalism, the Nazi salute, and the French revolutionary oath . . . " (p. 180), which, while it is meant to illustrate rituals in regimes of butchers who inspired fear, also mocks the notion of systematic comparison of cases. To put it another way, Kertzer seems to have no principle of selection for his examples and has made himself a prisoner of his own definition of ritual.

This book is illustrative rather than systematic. Its genesis lay in Kertzer's childhood experiences of the ritual surrounding JFK's funeral and the book concludes on the note that despite the many attempts to put politics on a rational basis, the rituals and rites of politics continue. In the end, the book is an intellectual and personal treatise about the importance of ritual in politics.

JOHN BENDIX

Lewis and Clark College

Religion, Morality, and the Law. Edited by J. Roland Pennock and John W. Chapman. New York: New York University Press, 1988. 298p. $42.50.

The reemergence of fundamentalism as a political force in the late 1970s has triggered an enormous interest in religion among political scientists in the late 1980s. As such scholarship has matured, articles and books have moved from a descriptive to an analytical or speculative orientation. Among the latter is this series of twelve essays.

Several essays struck me as having particularly useful and original insights. For example, Stephen Holmes's "Jean Bodin: The Paradox of Sovereignty and the Privatization of Religion" provides a novel summary of Bodin's *Republique*, namely, that even unvirtuous rulers should understand that a prince's power can be enhanced by acquiescing to laws of succession, nonalienating of the domain lands, customs, no taxation without representation, religious toleration, and so on. As he says at one point, "a cynic might even claim that the cause of religious toleration succeeded chiefly because it proved advantageous to power-wielders" (p. 6). The paradoxes relevant for contemporary society are that limited power is more powerful than unlimited power, religious toleration is sovereignty-reinforcing and irresolvable disputes are best handled by methods of avoidance (p. 38). Bodin is not usually read as a disciple of Machiavelli, but Holmes makes a persuasive argument.

The key chapter in the book is the late George Kelly's "Boyle's Commonwealth of Atheists Revisited." Facing squarely the fact that modern political theory, while accepting the dynamic interaction of law and morality, is distinctly uncomfortable with religion, Kelly asks whether it really is possible to have a society without religion. Would a society with law and a fully natural morality be enough? Taking as his starting point the book of an obscure French philosopher, Piere Boyle's *Pensees divers sur les cometes* (1682), Kelly questions the social utility of religion and goes through the litany of evils committed in its name. But in the end he concludes with Voltaire that "nature really has no plan, no capacity to give life meaning" (p. 100). For that reason religion remains—cynics, skeptics, and secular intellectuals notwithstanding—a powerful social force. It cannot be wished away. That may not be all bad; for while some would find a commonwealth of atheists congenial, Kelly doubts if they would sit comfortably, presumably because they will continue to be surrounded by those who do profess a religion. (Even the children of professional atheists have been known to get religion).

Easily the most straightforward, table-thumping essay in the book is Lisa Newton's "Divine Sanction and Legal Authority: Religion and the Infrastructure of the Law." The only woman among the authors, Newton asks bluntly, "Is religion necessary for law?" While her argument is far more nuanced and sophisticated than can be explained in a review, her conclusion leaves little doubt where she stands: "Indeed the law needs religion, although not the way it thought it did. It needs religion as the escape hatch for those driven to their limits by the unrelenting material pressures of the state and as the free space within which new ideas can incubate and as the protector of those ideas, when presently unwelcome, until the state can be taught to accept them. Above all, religion is needed as a counterweight to the vast and bumbling power of the state, as a glue for all that area of associative life that predates and is presupposed by the state, in which law operates most effectively and humanely" (p. 180).

While Newton seems sympathetic to the concept of religion as a mediating structure—a thesis put forward a decade ago by Peter Berger and Richard Neuhaus in *To Empower People* (1977)—she recognizes the diversity of roles religion plays in society and wants it consciously to choose as its major role the role of protector of the private sphere against state encroachment. Newton rejects the contention that law needs religion to maintain its own legitimacy, but her argument here is less persuasive. The power of Islam in Iran and Afganistan, Catholicism in Poland, Buddhism in Burma, and liberation theology in Latin America indicates that in the long run governments rule more easily if they do not alienate religious leaders or reject religious ideas.

Even the weakest article in the series, Louis Henkin's "The Wall of Separation and Legislative Purpose," is inadvertently useful insofar as it shows the silliness strict separationists get into when trying to exclude religiously motivated legislation as unconstitutional. It is probably the least illustrious piece Henkin has written.

Overall, this is a superior collection of essays. One curious inclusion is an unfinished essay by Robert Cover of Yale Law School, who died after completing three parts of a four-part essay, a case study of a Jewish attempt to bring the Messiah through law. Another weakness is a lack of serious attention to either the Islamic struggle to integrate religion, morality, and law or to liberation theology. Both provide challenging insights into the problem. Aside from these gaps, the book provides a rich panoply of ideas from an enormously talented group of authors.

PAUL J. WEBER

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The Concept of Liberty in the Age of the American Revolution. By John Phillip Reid. Chicago: University of Chicago Press, 1988. 224p. $25.95.

As Reid says, the emphasis of this study "is on words. To dwell on words may appear too narrow a focus, but words merit our attention because unless we are careful they are apt to mislead" (p. 116). Reid argues that inattention to the changed meanings of words has misled scholars of eighteenth-century political thought. He maintains that the meanings of words common in eighteenth-century political writing—*liberty, licentiousness, slavery,* and *property*—have changed and that twentieth-century analysis has often been insensitive to the changed meanings.

Reid's focus is upon the legal interpretations of liberty. He says that efforts to discover the source of the eighteenth-century debate on liberty overlook the obvious source, common-law constitutionalism. Both British and U.S. thinkers looked to the same constitutional sources for the basis of liberty. U.S. thought, Reid says, came from mainstream British thought.

Liberty was a motivating force in the eighteenth-century. It was "an abstraction that was both a standard and an objective goal, a concept so charged with content that it had utility"

(p. 20). The preferred claim for a right to liberty rested upon the notion that liberty was the inheritance of English-speaking peoples. The argument that liberty is an inheritance has led scholars to see a too-simple connection between liberty and property. Property includes liberty, not merely material possessions (p. 25). Reid braves the charge of "intellectual heresy" by suggesting that natural liberty (that is, liberty as freedom from all restraint) as a basis for civil liberty was regarded by most eighteenth-century theorists as little more than a theoretical explanation for liberty. Liberty without restraint could not be imagined outside of the state of nature (p. 30). The liberty of eighteenth-century discourse is civil liberty. To understand this, he maintains, it is necessary to separate rhetoric from argument. Legal restraint converts natural liberty into civil liberty; the freedom to act unless forbidden by law.

Eighteenth-century writers defined liberty by placing it in the context of its opposites: licentiousness, slavery, and arbitrary power. Licentiousness was the product of individuals disdainful of authority. British and Colonial loyalists saw danger in the licentious mob. Colonial whigs saw danger in unchecked governmental power. How people viewed political events helped to determine the side they supported in the American Revolution (p. 37). Slavery was one of the most frequent words in eighteenth-century political discourse. Chattel slavery, according to Reid, was not considered the worst form of slavery. The worst slavery was the loss of civil liberty. For the Colonial whigs, the tea tax, the Coercive Acts, and the Quebec Act were proof of arbitrary legislative power: slavery.

Reid concludes that much of what present-day readers of eighteenth-century political tracts think to be rhetorical excesses—for example, the use of the word *slavery* to describe the tendency of governmental actions and the connection made between *liberty* and *licentiousness*—are instead the result of misunderstanding eighteenth-century writers. He makes a convincing case. Only in the discussion of the meaning of *slavery* is the case pressed too enthusiastically. Granted that the use of *slavery* by political writers almost always refers not to chattel slavery but to arbitrary government, nevertheless, for some writers there was a connection, for instance, Jefferson's expunged reference in the Declaration of the "execrable commerce" of slavery and English essayist Samuel Johnson's observation that "we hear the loudest *yelps* for liberty among the drivers of Negroes." But this is a minor criticism. Reid's analysis does point to the need for a fuller understanding of the meaning of *slavery,* as well as the meanings of *licentiousness, arbitrary power,* and *liberty.*

This is an excellent book, well documented, with a detailed index. It is a logical outgrowth of the work of Rossiter, Bailyn, and Wood in their demonstration of the importance of ideology in the period of the U.S. founding. It is, as well, an extension of their work in that it directs attention to the need for a more profound understanding of the language that expresses that ideology.

DWIGHT LAMBERT

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Logic and Politics: Hegel's Philosophy of Right. By Peter J. Steinberger. New Haven: Yale University Press, 1988. 254p. $24.00.

A fundamental premise of this book—and one the author believes distinguishes it from most other works on Hegel's social and political philosophy—is that Hegel's *Philosophy of Right* must be read in the context of his larger philosophical system. When so read, instead of the "domesticated picture" of Hegel that one gets from those who would cast him in the liberal tradition, we find a "bolder and more radical" Hegel who challenges modern conceptions of constitutional government. Steinberger's strategy throughout is to show how the philosophical method of Hegel's *Logic* guides his derivation of the various social, political, and moral conceptions in the philosophy of right.

The book is divided into two parts, the first of which provides preliminary discussions of Hegel's basic problematic—the relation of the individual and society—and of his logical method. According to the author, Hegel differs from most of the other political thinkers of the modern period in his taking a "perfectionalist" rather than an "accomodationist" perspective on the relation of the individual to society. Instead of resolving the tension between individual and society by effecting an accommodation or compromise between the two in which one gives way to the other, Hegel shows how the two concepts can be reinterpreted so that the apparent incompatibility is removed. When properly understood, the concepts of individual (subjective freedom) and society (system of objective rules) are dialectically related, that is, the tension between the two is surpassed by their mutual dependence, in which each is perfected.

The tools for constructing this compatibility are found in Hegel's *Logic*, which explicates how the dialectical approach to a subject matter leads to overcoming seeming contradictions. Here dialectical reasoning is understood as a sort of "conceptual analysis" that relies on fundamental "metatheoretical" concepts to guide the search for intelligibility. Beginning with a description of apparently incompatible concepts, conceptual revisions in light of other "uncontested" concepts can produce consistency within the larger set. Moreover, Hegel's dialectic exhibits a conceptual necessity similar to that of standard logical inference, as well as producing conceptual innovation in its unfolding of the rational structure of reality.

In part 2 Steinberger shows how Hegel's logical method is at work in the derivation of various institutions that comprise the rational state, in particular the institutions of punishment, marriage, and monarchy. The overall context of discussion is the concept of right, that is, "the entire normative structure of social life." The purpose of Hegel's *Philosophy of Right* is "to trace out the logical and immanent development of right in the world and to uncover thereby the underlying relatedness of all social norms, from the simplest to the most sublime" (p. 121).

The author shows how the institutions of punishment and marriage can be derived from considerations of how the free rational self must of necessity relate to objective social rules. To act freely and rationally is not simply to make an unconstrained choice but to see that one's subjective choices are dependent upon a substantial social order. Individual and society are here reconciled through the perfecting of each: abstract selves become concrete persons through self-conscious integration into higher relations.

It is in the organic conception of the state that the tension between individual and society is ultimately dissolved, for only on this conception—following Hegel—can the claims of the individual and those of society be fully satisfied. This presupposes that each member of the state actively reflects the whole in some way, that each member is free, and that each is conscious that his or her own identity is bound up with the state.

The derivation of monarchy in Hegel's *Philosophy of Right* is one of the more provocative discussions and proceeds from a consideration of the organic character of the state. If the rationally constituted state is to be more than a mere collectivity, subjective freedom must be had not only by the individual members but also by the whole. Political society itself is an agent only if sovereignty is located in the will of a single individual—the monarch—who acts absolutely for the whole and yet without diminishing the pluralistic constitutional structure upon which the monarchy is based.

Although Steinberger argues that Hegel's position is in principle a defensible one, he does point out some "structural flaws of a philosophical nature" in Hegel's project (pp. 234ff.). These problems seem to center on the difficulty of reconciling the requirements of freedom in an organic conception of society with the realities of political power. Overall, however, Steinberger does a very credible job of explicating Hegel's philosophy of right and arguing for its overall plausibility. The text is very clearly written and is of great assistance in piercing through the esoteric character of Hegel's thought.

DAVID A. DUQUETTE

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Community and Alienation: Essays on Process Thought and Public Life. By Douglas Sturm. Notre Dame: Notre Dame Press, 1988. 256p. $28.95.

Sturm claims to develop the implications of the tradition of *process* thought deriving from Alfred North Whitehead for social and political life. Ten essays, originally prepared for different occasions, ply from various angles the common thesis that the politics of process thought is "communitarianism" rather than "individualistic liberalism."

Sturm argues that classical liberalism, with its conception of individuals as morally complete beings who must tolerate public life only as a means to secure fundamentally private interests, rests on a metaphysics of substance. Entities are self-sufficient, substantial essences that enter into purely external relations. Process philosophy displaces this metaphysics with the principle of internal relations, the doctrine that "everything in the universe is conjoined with everything else in a continuous process of becoming," that "apart from its relationships, an entity cannot be what it is" (pp. 12, 34). On this view, the human self is thoroughly social, being constituted by its relations to the natural environment, other persons, and its own past and future. The normative bearing of this ontological fact is that the purpose of public life is "to create and sustain those relationships in which the actions of each enhance the life of all" (p. 21). Modes of public life that deny the communal basis that is "a natural part of our existence," producing "negative form[s] of belonging," are forms of alienation (pp. 3, 5). In several of the essays, Sturm reflects, from this general perspective, on more specific topics, including methodology in political science, corporate culture, capitalism, legal realism, and bioethics.

Sturm successfully challenges previous interpretations of process philosophy—especially those of Samuel Beer and Allison Johnson—that aligned it with rather classical conceptions of liberalism. His critique of liberalism is also worthy on its own terms, although some of the evils he identifies as "curious transmutations" of individualism ("corporatavism, racism, and nationalism," p. 1) are surely as much the product of enthusiastic collectivism as of strident liberalism. Some of the discussions incidental to his major thesis are provocative as well.

The book is disappointing, however, in failing to show what distinguishing features process philosophy brings to a communitarian perspective. Sturm contends that the Marxist conception of alienation is "compatible with process thought" (p. 32). Indeed, his own definition of alienation as "an institutional pattern in which a people suffers from the pernicious consequences of its own life activity" (p. 23) faithfully reiterates Marx. His call for participatory social and economic democracy follows the same line. But one expects his perspective, rooted in a philosophy that emphatically rejects a materialist metaphysics, to differ from Marxism in accounting for how negative forms of belonging, with their "inertial tendencies" (p. 5), arise in the first instance. One expects a different conception of human nature. Sturm cannot return to the simplistic psychology of the purely self-interested ego that underlies liberalism, of course, but a harder gaze at the principle of internal relations itself might disclose alternative conceptions. Samuel Alexander—another process philosopher and a contemporary of Whitehead—remarked that the nature upon which we are so utterly dependent both nurtures *and* finally destroys us. Add to this the recognition that our mutual dependency on other finite selves entails mutual inadequacy to each other's purposes, and forms of alienation become explicable as manifestations of deep resentment against our radical finitude.

Instead of exploring this unhappy but ineluctable underside of the principle of internal relations for root causes of alienation, Sturm vaults repeatedly to theology to ground his faith that alienation, whatever its source, can be overcome. He argues that God, "intends a universal community of being" (p. 186), "unifies and preserves the world" (p. 85), and functions as "the Ultimate Efficacy in the creative passage of events" (p. 2). In appealing to God as the guarantor of an inevitable impulse of life towards perfection, Sturm finally departs from Marxist and other nontheistic communitarian perspectives. But this difference derives not so much from the principle of internal relations in itself, which Sturm places at the center of process thought, as from the influence of the school of theology that has virtually taken over process philosophy in the United States.

Community and Alienation is liberation theology for the First World. The book contributes to the internal debate among process theologians as to what politics befits their religious doctrine. For those with a different conception of God or who believe that He has long abandoned the world, it provides few new suggestions for the establishment of genuine community.

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Politics, a Work in Constructive Social Theory. By Roberto Mangabeira Unger.

Cambridge: Cambridge University Press, 1987. Vol. 1, **Social Theory: Its Situation and Its Task.** 256p. $39.50 cloth, $12.95 paper. Vol. 2, **False Necessity: Anti-Necessitarian Social Theory in the Service of Radical Democracy.** 635p. $59.50 cloth, $18.95 paper. **Plasticity into Power: Comparative-Historical Studies on the Institutional Conditions of Economic and Military Success.** 231p. $37.50 cloth, $10.95 paper.

Roberto Mangabeira Unger first came to the attention of political theorists with *Knowledge and Politics* (1975), a tour de force critique of liberalism from a young but philosophically mature Harvard law processor. While critiques of liberalism have become a sort of academic cottage industry, Unger's was magisterial in scope, creativity, sensibility, and eloquence, leading the reader through the dense layers of antinomy, contradiction, and sterility that constitute classical liberalism. His concluding words—"Speak, God"—directed readers to the passionate hope that the political equivalent of religious love—community; a common good; a shared, intelligible telos—would somehow take hold in the affairs of men. At the least, Unger had deepened our understanding of our peculiar predicament as social beings longing for the transcendent.

The three volumes of Unger's new study, *Politics*, probably will come as a shock to those familiar only with his first book. As intervening work makes clear, particularly *Passion* (1984), Unger's is now a project of unabashed personal liberation, based on an ontology of endless dialectical reciprocity and the lack of the possibility of discerning an objective political end. The active struggle of perpetual human re-creation in whose midst glints the tyranny and violence of anarchism—allowed when freedom is anointed the status of substantive good—is now substituted for the silence of God. Unger argues that political struggle (and its corollaries creativity, instability, and renewal) is redemptive of our best human possibilities and not merely a reflex born of eternal frustration with a finite and imperfect world. Unger is a fascinating and important thinker, but the move he makes in *Politics* will disappoint early admirers who awaited from Unger a more detailed and substantive account of communitarianism (the realization of man's species nature in concrete forms of life), the perpetual desideratum of political theorists. As it turns out, "communitarianism" becomes but another "context" to be smashed in the perilous radical project of superliberal liberation.

Social Theory is the "critical introduction" to *Politics*. In it Unger, with dazzling rhetoric, prepares the familiar case for a radical leftist social theory by threading a rhetorical path between forms of necessitarian social theory (Marxism as chief exhibit) and the fluidity of liberal democracy with its masked elite entrenchment. He eloquently challenges two competing views of society: that politics is frozen (by nature or historical law) and that it is utterly fluid, with no toehold for universal emancipation. The ideal of personal and political liberation is termed the "radical project." And this project is "morally perilous" since it must be engaged in at the risk of unleashing dark potencies and the dive into nihilism. Unger writes, somewhat casually, of the radical program, "At worst, the practical and imaginative strife it unleashes overshadows the attempt to promote and to reconcile the different facets of human empowerment. At best, many loyalties embedded in established contexts of social life must be betrayed" (p. 214). Nonetheless, the "imperative of plasticity" must be accepted if human affairs are to have any chance to be creative and emancipatory.

At the heart of Unger's narrative is what he calls the paradox of contextuality, "our need to settle down to a particular context and our inability to accept any context in particular as fully satisfactory" (*False Necessity*, 342). And yet it is clear how Unger plans to explode that paradox. The radical project requires that the "constraints of society" yield to the original, innovative subject we all believe ourselves to be. If society is an artifact, it can be remade. Necessitarian social theory is simply a powerful illusion. Once given up—as the cause of our empowerment requires—we are free to attack any and all societal constraint.

Unger is commendably frank about the acceptance of radical plasticity. That the project may spell the descent from exhilarating liberation to servile domination Unger accepts unflinchingly. The modern vision, echoed so well in Sartre's *Critique of Dialectical Reason*, of endless seriality and oppression—interrupted by moments of fantastic freedom—is a prospect that does not faze Unger. He writes, sanguinely, that "each advance toward greater plasticity and disentrenchment creates new dangers of reversion to less revisable and more oppressive orderings of social life" (*Social Theory*, 213).

Plasticity, then, despite its risks is a normative ideal and a societal necessity. *Plasticity into Power*, a selected social history illustrative of his thesis in the dynamics of state building, preaches the gospel of plasticity and the powerful military and economic advantages of the plastic state. Put bluntly, power goes to those most willing and most able to smash their inherited contexts. And yet, disarmingly, at the end of that volume Unger suggests that plasticity is not a good in itself, that it must be harnessed to a higher social ideal. This is a conclusion for which the reader is unprepared; *False Necessity*, the centerpiece of these volumes, is nothing if not persuasive that Unger intends plasticity to be understood as the highest achievement of human action, unsubordinated and descriptive of authentic political life. Unger's practical project is what he calls "trashing the [social] script" (p. 319).

Through a taxonomy of political traditions—liberalism, Marxism, libertarianism, communitarianism—Unger selects an aspect of each to endorse. Communitarianism, for example, rightly "identifies the great wound of modern societies as the disruption of communal bonds that place each individual securely within a network of reciprocities" (p. 23). But prevailing institutional arrangements are not an adequate vehicle for expressing the communitarian ideal. To accept an underlying institutional framework—even one tinkered with—is to accept a "social script," and this acceptance endangers and impoverishes the legitimate communitarian hope. The impoverishment "lies in the representation of community as a protected haven from which conflict is banished rather than as a zone of heightened mutual vulnerability in which people may entrust themselves more fully to one another, whether they conflict or agree" (p. 24). These are seductive words, resonant of a minor chord of reconciliation in Unger's treatise. But does his institutional program convincingly deliver on this promise for an expansive conception of community?

Unger rejects the program of social democracy, despite its inherent humane virtues, because it unduly constrains the means of heroic emancipation, empowerment, and economic plasticity. "We are," he says simply, "too little under social democracy" (p. 27). "Empowered democracy," Unger's choice, flows from the exercise of Unger's master concept: "negative capability" or "empowerment through disentrenchment" (p. 249), the emancipation of social action from false necessity and the "narcoleptic routines" (p. 279) of history by continually putting the basic institutional structure of society "up for grabs". Unger adumbrates theories of power-free markets and abolition of capital inheritance; a confiscatory state reclaiming accumulated wealth whenever it becomes concentrated; lending banks investing public monies in teams of entrepreneurs, workers, and technicians; public control of fundamental investment policy; and the multiplication of overlapping powers and functions of the state to prevent hegemony. Finally, he envisions an entirely new branch of government devoted mischievously to social destabilization through "systematic interventions" into "every aspect of social life and every function of all the other powers in the state" (p. 453). It is not clear how this institution would be safeguarded from rigid tyranny, or indeed why it too would not be a context to be smashed and destabilized.

Recalling Kuhn's famous distinctions, Unger is intent on replacing "normal politics" with the breakthrough conditions of "revolutionary politics." His context-smashing constitution will prevent the liberal tendency of relapse into "a fantasizing, sentimental, archaic, tyrannical prospect of devotion to a shared vision of the common good." Unger anticipates the criticism that a total destabilization of society and hierarchy could throw the citizenry into "an endless anguish of envy and longing" and "invidious comparison" and rejoins rather weakly that this prospect is largely moot, primarily because his empowered democracy "presupposes and makes possible a major equalization in the material circumstances of life" (p. 472). The belief that radical deracination will not breed resentment and envy if only material inequalities are minimized is theoretically convenient and alarmingly optimistic.

The deep issue addressed is that of "security" and "community," of being rooted in a certain place and vocation, in a context of particular reciprocities. Under Unger's institutional program, security is not based on a guarantee of a particular job or the consolidation of property rights and the myriad relations engendered by a specified form of life. Rather, security is developed "in part by finding senses and varieties of security compatible with an ever greater jumbling up of distinct styles of life and in part by awakening to a conception of the personality as both dependent upon context and strengthened through context smashing." The point is to "diminish the extent that safeguarding security rigidifies social life and thereby helps reproduce inflexible roles and ranks" (p. 515). Unger admits that this vision of empowerment endangers traditional claims of community, submerging "concern with trust under the power-mad or narcissistic flaunting of the will and the heartless cult of magnificent capability" (p. 592). But he also notes certain benefits of the radical project, especially that institutionalized destabilization allows us to be close to others without being subservient. Thus the empowered realize the enlarged generosity toward others that is the "mark of love" (p. 594).

What is one to make of this recondite, poetic, visionary work—1100 dense pages reverberating with the same exhortation: "smash" contexts, "trash" the social script, "raise a storm," "liquefy," and put everything "up for grabs" in a "heightened condition of plasticity"? These seem tired ideas, albeit cloaked in brilliant and inspiring language. Unger's prescription for liberation from unjust structure (well, *any* ongoing structure) seems exasperatingly romantic and self-indulgent. Do we need reminding that smashing a context—delightful as that may be from time to time—is certainly no guarantee that its replacement will be superior? One wonders, reading these volumes, in what "context" Unger himself is situated and why that context itself is not offered up to the sledgehammer. There is an eerie sense of ahistoricality pervading this work, as if Unger himself is not situated in some very particular and much-argued intellectual traditions. But what ultimately frustrates is Unger's acquiescence to a gleeful experimentalism—as if the profound context smashing this century has ushered in were just a docile experiment to its victims. The postulate of a radical discontinuity with past and tradition harbors unsettling implications. Is it right to view contexts, rules, and habits as always disabling? Are they not, in fact, the essence of any meaningful social life? A careful dialectic of creativity and history—typical of the earlier *Knowledge and Politics*—is, despite a nod here and there, abandoned to the brute self-assertion of questionable liberation.

By criticizing Unger, one need not discourage our particularized traditions from engaging in the free play of imagination and its enactment in our politics. One is not rendered imaginatively sterile by recognizing a tragic perspective on human affairs that faces up to the tensions of our awkward lives together. The tragic mood need not issue in passivity or induce conformist quietude. It may just as well promote faithfulness to the extreme richness of the world, to the fragility of human existence coupled to a loathing and reluctance to wreak harm in the world.

We are dialectical beings—caught forever in the condition of creator and created. One need not abandon the insight that we *make* our own history (and in turn are made by it in surprising and unexpected ways) to mute the ambition of Promethean creativity. Authentic political life is the dynamic product of creative projects that acknowledge the tensional character of human existence: its creativity is channeled by an ontology of tradition and history that can be modified and extended only from within. There are ties to a collective creation, avenues to past achievements that generate new projects on which creative, progressive politics can be practiced. In our links to others and the world—in recognizing a shared complicity—we may find both the "greatness" and the "sweetness" Unger so righteously desires.

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The Company of Critics: Social Criticism and Political Commitment in the Twentieth Century. By Michael Walzer. New York: Basic Books, 1988. 260p. $19.95.

In this book Michael Walzer gives flesh and blood to the view of "connected criticism" he has endorsed in recent books. He seeks both to concretize this method and explore the practical problems confronting it and to do so based on the sensible premise that "questions like these are best answered with reference to the life and work of actual critics" (p. ix). The book is thus constituted by a series of fascinating excursions through the careers of 11 twentieth-century social critics, all "representative figures, whose work is touched by the triumphs and catastrophes of our time: the two world wars, the struggles of the working class, national liberation, feminism, totalitarian politics" (p. 27).

Walzer opens with an introduction laying out the main themes of the book and challenging two myths of social criticism. The first is the myth of theory, the belief that it is the proper task of social criticism to abstract from particular problems and locales and create specialized languages purporting to articulate universal moral truths. The second is the heroic myth, the romantic stereotype of the critic as alienated thinker, independent of all social ties and historical contexts, pure seeker of truth. In the modern world these myths have produced the ideal of the social critic as disconnected theorist, whose task is to remake the world in the interest of the truth. Such an ideal has had liberal and socialist incarnations, the most troubling of which for Walzer is clearly Stalinism. Walzer believes that this ideal has failed both intellectually and politically and that it has also invidiously misrepresented what the best social critics have sought to accomplish.

Walzer's profiles of his subjects are uniformly readable and interesting, and some are exemplary contributions to intellectual history. In the best tradition of criticism, each constitutes a short morality play, putting to the test the character, sensibility, and insight of the critic in question. As those familiar with Walzer's work will not be surprised to learn, his heroes are Ignazio Silone, George Orwell, Albert Camus, Martin Buber, and Breytan Breytenbach. Each of these thinkers managed to join penetrating criticism of the injustices of his society with a powerful identification with its traditions and peoples, and each approached his subject soberly, with a sense of limits, and in an effort to encourage dialogue and plain human dignity.

There are no villians in Walzer's account, but there are a number of writers whose work and whose politics are found wanting—Antonio Gramsci, Simone de Beauvoir, Herbert Marcuse, and Michel Foucault. In each of these cases, Walzer insists, the critic is so distanced from his society in its concreteness as to lose a sense of historical possibility and lapse into cynicism. Walzer makes a powerful case for "connected criticism" here—or rather his heroes make such a case, in virtue of their undeniable insight and integrity. And yet Walzer's account is not without its problems. First, while he focuses upon the motivations, sentiments, and affiliations of his critics, glaringly absent is any discussion of the *truth* of their claims. This is consistent with Walzer's disavowal of "objective" political theory and with his disinterest in epistemology, but it is troubling. Is it enough for the critic to make a complaint, however authentically inspired and colloquially expressed? Don't we need to *justify* our criticism, in the light of certain truths about human beings and about the circumstances they find themselves in?

Second, Walzer insists upon "connection" to one's culture and community. But whether we think of ourselves as "Americans," or Jews, or socialists we can never be unequivocally determined by the concatenation of circumstances that constitute our society, however much it may be influenced by them. And, once we establish our own identities, we still face the problem of how to interpret them. What does it mean to be a real "American"? Randolph Bourne thought it meant that we should oppose militarism and war. Teddy Roosevelt thought it meant that we must support them. It is one thing to insist that the social critic must have a feel for, and concern with, the particularities of the society criticized. It is quite another to suggest that such a feel validates, or even determines, the character of the criticisms. I would suggest that the power of Walzer's arguments about connectedness presupposes precisely the values that need independent justification.

Third, Walzer tends to present an unfair stereotype of Marxism. Many of his criticisms are undeniably compelling. But I believe that more needs to be said here about the heterogeneity of Marxism as a tradition, and about the possibilities of *Marxist* "immanent criticism."

Finally, an ambiguity needs clarification. Walzer persistently emphasizes the critic's ties with existing communal, national, religious traditions. One occasionally almost gets the impression that once we have been awakened from ideological nightmares, these existing identities will be a sufficient foundation for our moral commitments. Yet this is deeply problematic. Certainly the author of *The Myth of Sisyphus* and *The Rebel*, who testified to an "absurd universe," would not accept this. Neither, it seems, does Walzer. He seeks a pluralistic moral perspective that would incorporate our best moral intuitions and our vital communal traditions and allegiances in a way that necessarily transforms them. But more needs to be said about the limits of our existing attachments.

Walzer has brilliantly emphasized one side of the theoretician's dilemma, that of attachment to the familiar. But we also need to emphasize the other, equally important side, that of detachment and theoretical abstraction. In doing so, however, we can ill afford to ignore the important lesson Walzer has taught us—that social criticism requires a sensitivity towards the concrete, the local, and the experiential and that any conception of justice worthy of the name must accept the inevitability of human difference and must tolerate—indeed celebrate—plurality. In delivering this lesson, Walzer has proven himself to be not simply an insightful interpreter of the best of our critics but an accomplished member of their company as well.

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Cicero's Social and Political Thought. By Neal Wood. Berkeley: University of California Press, 1988. 288p. $35.00.

A systematic study of Cicero's political thought is long overdue. Perhaps no other major figure in the history of political philosophy is more in need of systematizing, for Cicero seems to embrace opposites—the hero and the coward, the villain and the martyr, the man of action and the man of thought. It is the constant temptation of Cicero scholarship to simplify this complex figure, a temptation too often indulged in recent decades. Neal Wood's study is a quantum improvement over the one-dimensional portrayals of the past.

Necessarily, Wood paints his subject in large strokes; the sheer volume of the Ciceronian corpus renders impossible a close textual exegesis of each of his writings. There is therefore an abundance of points on which a critic might hang an argument: a generalization that might be more carefully documented, a characterization that might be more fully fleshed out. The complexities of Cicero's friendship with Atticus, for example, remain largely unexplored. Wood presents the friendship as primarily a business partnership. Curiously, in a volume that emphasizes Cicero's gritty political realism, he does not mention the incalculable political value of Atticus' "friendship" to a rising young politician like Cicero.

Wood's most important theme is that Cicero anticipates many of the subjects of modern political thought. Not only does Cicero share Machiavelli's hard-hearted realism, he prefigures Hobbes's separation of state and society and Locke's emphasis on property: "For Cicero the state exists primarily to safeguard private property and the accumulation of property, not to shape human souls according to some ethical ideal of the virtuous" (p. 11). Yet in the chapter devoted to this theme, the contention is supported by evidence from Cicero's political works—his orations and letters—rather than from such works as *De Republica* or *De Legibus* in which "ethical ideals" are given greater weight.

The difficulty is this. Cicero may have intended for different kinds of writing to be read or heard by different audiences. No law professor today would speak in the same way to a jury as he would to his law students, or as he would to another law professor. Cicero was more than anything else the supreme master of rhetoric, that is, of speech or writing that would move an audience. This mastery is the solid foundation of his fame, even as his philosophic works are, arguably, its pinnacle. Mastery of rhetoric means knowing one's audience and knowing what will move it. The views expressed in works intended for a limited, aristocratic readership may differ from those intended for a public forum. To which audience would Cicero reveal his "true" teaching? Perhaps in the end we will discover a unity, yet if we conflate these categories without first exploring their surface diversity, we risk oversimplifying Cicero's thought. Unfortunately, Wood does not tell us how he reads Cicero's works or how he understands the apparent differences between them.

For example, if *De officiis*, Cicero's last major work, is—as it is said to be—the source of his "conceptual separation of state from government, his beginning distinction between state and society, and his notions of trust of government, tyranny, and tyrannicide" (p. 68), how are we to understand its relation to his previous work? Wood's interpretation is a direct challenge to the conventional scholarly wisdom, which holds that Cicero merely popularized the views of the Stoic Panaetius. Certainly Cicero's criticisms of the Stoics in the *Tusculan Disputations* and *De finibus* would be relevant here as evidence of Cicero's attitude. Yet although both works are relatively late and hence nearly contemporaneous with *De officiis*, neither is considered in the text.

Wood cannot be faulted for failing to include the favorite theme of every critic. This is an important work, not simply because it is the first of its kind but because it presents the complex character of this scholar-politician and the relation of his life to his writings in greater depth than any recent study. It is an impressive synthesis of postwar English and U.S. (although not, to judge from his bibliography, Continental) scholarship on Cicero. It establishes a benchmark in Cicero scholarship by which later treatments will be measured. It is, moreover, courageous, for the book will now be the target of all who think they can improve upon Wood's understanding. But they will all have to read this thoughtful book first.

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Commission on the Bicentennial of the U.S. Constitution

AMERICAN POLITICS

Rationing Medicine. By Robert H. Blank. New York: Columbia University Press, 1988. 280p. $25.00.

The spiraling cost of health care in the United States has prompted policy analysts to raise two important and related questions: How do we control health care costs and who should receive the care? Robert Blank's book ambitiously addresses these issues, arguing that the emphasis on curative health care in the United States needs to decrease as the usage of preventive health care increases.

Chapter 1 argues that the focus on curative and technologically intensive medical care, an aging population, and government involvement in paying for medical care are at the root of spiraling health care expenditures. While cost escalation has led to attempts to control expenditures, these efforts are often pursued in a fragmented fashion and have led to the development of a two-tiered medical system and a lack of developed societal goals that could be used as a basis for rationing and allocating health care.

Chapter 2 analyzes three areas of curative medicine where these cost and rationing problems are pervasive: organ transplantation, neonatal intensive care, and fertility and reproductive technologies. These procedures are expensive and rationed, with the policies used for rationing being implicit, fragmented, inconsistent, and inequitable (chap. 3). People who can manipulate support for their care (e.g., nationally publicizing a child's need for a liver transplant) and people who have access to health insurance through their employers have better chances of receiving care. Even in the latter case, the question of what health care an individual should receive is determined by individual employers.

Blank feels that the key to establishing a consistent and equitable system for allocating health care lies in changing public attitudes so that preventive health care and healthy lifestyles receive emphasis over curative medicine. Because this change in priorities will not occur "without vigorous public support—support that will not be forthcoming unless the public has substantial input into the policy being made" (p. 174), chapter 4 argues for facilitating public education and public participation to increase the emphasis on preventive health care as a goal. Blank argues that a positive right, a claim by an individual on others to provide health care, should be balanced with the individual's responsibility to refrain from behaviors that put them at risk. Individuals who engage in such behaviors "should be prepared to relinquish their claim on societal health care resources" (p. 195).

Blank's argument assumes that many expensive, curative medical procedures are often an outcome of individual choice. While this argument makes sense for some health problems (e.g., smoking related problems), generalizing the argument to other areas, such as neonatal intensive care, is difficult. Should an infant be held responsible for the behavior of its mother? Similarly, should a child be held responsible for parental decisions about diet?

There is also a tendency to rely on debatable assertions to buttress arguments. In support of his emphasis on using preventive health care to limit future expenditures, Blank states that "many, though not all, preventive health care programs are cost effective" (p. 123). His argument that health care rationing should take into account individual responsibility for one's own behaviors that affect health seems to be based on the assertion that "most, although certainly not all, health hazards today are avoidable by individual action" (p. 202). Similarly, he states that "the greatest gains of the future promise to come not from medical research but from individual efforts to be healthy" (p. 132). While these assertions are not necessarily incorrect, they are offered without supporting arguments or citations. This is troublesome because others (e.g., Louise Russell's *Is Prevention Better Than Cure?* [1986]) and even Blank himself suggest that these assertions are not necessarily valid. In fact Blank states that there is a "paucity of conclusive scientific evidence on the deleterious effects of many lifestyle practices on health" (p. 204). Because these are key components of Blank's arguments, more space should have been devoted to demonstrating these points.

The arguments would also be strengthened by providing more complete economic analyses. While the medical procedures discussed in chapter 2 are expensive at the unit level, the societal cost might not be a major component of health care costs in the United States if the number of people obtaining the service is low. Thus, while there are potential savings to be generated, they might not be immense. Because Blank recommends that substantial and expensive actions need to be taken to educate and involve individuals, it seems fair to ask how much would be saved and how much more equitably health care resources would be allocated. Pursuing these issues in more detail would have strengthened the book.

Finally, the arguments are divorced from their political setting. Members of the coalitions supporting healthy life-styles include "commercial insurance companies, public insurance programs, and employers, who want to use the new evidence about lifestyle and health (however questionable) to lower their costs" (Deborah A. Stone, "The Resistible Rise of Preventive Medicine," *Journal of Health Politics, Policy, and Law* 11(1986): 676). A discussion of which interest groups support using healthy life-styles as a rationing device might illuminate who stands to gain from adopting new standards.

The problems are partially due to the ambitiousness of the project and could be remedied by limiting the scope of the book to health care problems affected by individual choice. Within this scope, Blank's arguments raise some interesting ideas about what criteria should be used for allocating scarce resources. These ideas need to be pursued more thoroughly.

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Private Interests, Public Policy, and American Agriculture. By William P. Browne. Lawrence: University of Kansas Press, 1988. 294p. $29.95 cloth, $12.95 paper.

Browne has written on a fascinating topic, the politics of U.S. agricultural policy. His book blends a carefully cultivated understanding of interest group research with a sensitivity to the particular issues and character of agricultural policy making. The work in particular expands and complicates our understanding of the role of interest groups. Browne finds a variety of approaches and strengths exhibited by organized interests in shaping policy, each targeted to selected legislation or executive decision. Four different types of groups—multiple interest, single issue, protest, and single project—are examined. Each performs its policy role in relation to the sectors of the public and industry it attempts to serve. Each is limited in its activities by its constituencies.

The book's analytical bent also constitutes its weakness. Browne assumes an understanding of issues reasonable for a trained political scientist but unlikely for otherwise interested readers, such as agricultural specialists, other social science professionals, or undergraduates. In addition, he addresses a number of interest group and democratic theory issues in ways that fall short of the detailed and careful analysis one might expect from a carefully researched, statistically sophisticated report. Moreover, his historical and contemporary discussions of interest group activities leave the reader overwhelmed with fragmented information about a myriad set of organizations.

A test of Browne's analysis of agriculture interest groups and their role in policy is provided by his exploration of the political maneuvering behind the 1985 Food Security Act, a five-year extension of U.S. basic farm legislation established in the 1930s. In spite of the Reagan administration's free market ideology, this bill actually extended the role of the state with greater authority for set-asides to control production, protect the environment, and subsidize exports. Recapturing lost market shares was particularly important even though the period of the overvalued dollar had ended. As he suggests, in spite of the bill's use of the word *Security* in its title, it might better have been called the "Farm Product and Export Act of 1985" (p. 230).

The main thesis of the book is upheld by Browne's case study. Congressmen and commodity interest groups do wield the most influence on farm legislation; and farm interests, as they construe them, do take precedence over more diffuse national interests. Thus, though the 1985 legislation defied Reagan ideology, it was accepted quietly by Republican leaders. The "fragmentation" of the farm lobby into dozens of different groups, tactics, and goals allows those focused on select specific interests to be more successful.

The book deals very little with internal dynamics inside Congress or the executive branch. It has no systematic overview of the farm situation and discusses details of farmers' problems in the context of describing specific groups—such as the Farm Bureau vis-à-vis extension services or the American Agricultural Movement (born when producer prices plummeted in the late 1970s) vis-à-vis grass root direct lobbying. There are almost no vignettes about personalities or emotional episodes in this account of farm policy formation. The result is rather dry reading. For readers who have never talked with lobbyists for the Cattlemen's Association or the National Wheat Growers Association, there is little feel about the style or mindset of such important groups. Browne clearly grasps the frame of mind and styles of organization in the farm "industry" but conveys little of it in his book.

Browne makes clear that simple schemes of U.S. interest group behavior are simpleminded. They miss the rich diversity of organized interests at play—at least in the farm legislation domain, an area where government intervention has long been strong. He also makes clear that narrow, focused interests do best in getting their way, while organizations that appear economically powerful—such as the Farm Bureau or major corporate bodies like Archer-Daniel-Midlands—may be fairly impotent. Often these multipurpose groups are cross-pressured by competing interests. The result is that policy, as seen from the interest group perspective (based on the author's extensive interviewing), leads to policy outcomes not related solely to group strength. With greater inclusion of the executive and legislative perspective the thesis about fragmentation could be strengthened (or disconfirmed). Unfortunately these other players are less fully studied in the account. Assuming they played only a passive role, the inertial forces maintaining agricultural policy seem to be a product solely of the particular configuration of the interest groups.

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Bureaucratic Politics and Regulatory Reform: The EPA and Emissions Trading. By Brian J. Cook. New York: Greenwood, 1988. 165p. $37.95.

In 1977, at the Carter administration's inception, reformers in the Environmental Protection Agency (EPA)'s Office of Planning and Management attempted to introduce major changes in the agency's approach to air pollution regulation and provoked what Brian J. Cook describes as a protracted "battle . . . over the professional heart and soul of the agency" (p. 101). The battle, continuing through the first Reagan administration, was fought over a bundle of reforms—offsets, "bubbles," banking, and netting—whose purpose was to substitute regulatory strategies based on economic incentives for the traditional command-and-control approach to securing compliance with air quality standards prescribed under the Clean Air Act.

The author's purpose is to describe "how the fundamental organizational characteristics of the EPA shaped the direction reform took in the agency" and to use the case study to provide "some admittedly speculative guidance on organizational change and policy reform to individuals with a keen scholarly or practical interest in the area." Cook's detailed and readable account, which relies heavily on interviews with major participants in the reform battle, also contributes to the relatively scarce but valuable literature on the institutional history of the EPA. The initial chapter summarizes the book's organization and theme. The next three chapters describe the major issues involved in the debate over economic regulatory incentives and trace the development of this approach to the Clean Air Act in Congress and the EPA. Chapters 5 and 6 are the most theoretically important: they describe the internal conflicts over economic incentives within the EPA and identify the organizational and ideological causes. The final chapter provides the major conclusions.

The author concludes that the EPA has operated "with a fundamental schism in its professional ranks regarding . . . the agency's mission, about the proper organization and purpose of a regulatory system, and about the appropriate instruments for achieving environmental policy objectives" (p. 124). He asserts that the EPA's ideological schism forced the reformers to create new organizational structures to implement the new regulatory strategies. Despite difficulties, Cook believes that reform leadership was successful in achieving some of its goals. Finally, he emphasizes the importance of ideology in shaping the response of different factions within the EPA to regulatory reforms. Throughout, the author maintains a strong linkage between the narrative and the literature on organizational behavior. Particularly valuable is his analysis of the relationship between organizational structure and agency response to policy implementation. Because the discussion is also well-grounded in both theoretical and partisan literature on economic incentives as a regulatory strategy, it offers useful insights into the opportunities and limits of this approach based on the EPA's experience.

The major problem with the analysis, inherent in the case study method, is the difficulty in generalizing beyond the events described. Thus, any "lessons" to be drawn are, despite the author's assertions, tenuous. Also, there is confusion in the concluding chapter over the long-term impact of the reforms: they are first described as "nudging the policy apparatus in a new direction" and later as "problematic" in their success (pp. 131, 139). Also, one wishes the author had pondered a bit about how his respondents' bias might impinge on his interpretation of events. Nonetheless, the book raises important issues. The material is a substantial and original contribution to the literature on the EPA's role in environmental regulation. The succinct and lucid discussion will be particularly appealing to readers concerned with the policy process, organizational behavior, political economy, regulatory reform, and environmental policy.

WALTER A. ROSENBAUM

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Political Voice: Citizen Demand for Urban Public Services. By Philip B. Coulter. Tuscaloosa: University of Alabama Press, 1988. 105p. $14.95 paper.

Citizen-initiated contacting of public officials has attracted the interest of urban scholars for nearly two decades now. Typically, the focus of interest is either (1) accounting for differences in contacting propensity across individuals and geographic areas or (2) exploration of local government's responses to contacting. The former is the more dominant in the literature, in part because it derives from a strong research paradigm on the correlates of political participation, and in part because it involves an intriguing and increasingly complex puzzle that is as irresistible to many scholars as a good murder mystery is to many readers.

Philip B. Coulter's book, *Political Voice*, falls squarely within the former tradition. Aside from some very limited attention to the results of contacting from the citizen's point of view (i.e., evaluations of the city's response), the study is exclusively devoted to the puzzle of explaining why some neighborhoods generate more contacts than others. The puzzle has become all the more intriguing because of the generation, within the last 15 years, of competing explanations of contacting propensity and conflicting research results, some based upon survey research on individuals and some based on analysis of official data on contacts from city subareas. An initial chapter of *Political Voice* lays out these conflicting results and competing models. The conflicts by and large focus on the role of socioeconomic status variables as predictors of contacting. Some have found a positive association, consistent with standard models of other forms of political participation. Others, however, have found negligible or negative relationships between socioeconomic status and contacting, while yet others have documented a curvilinear association, with contacting propensity highest in neighborhoods at the middle range of the social well-being scale.

But why the tangle of conflicting results? In the process of sketching the theoretical models attached to the conflicting findings, Coulter provides hints at some of the answers that have been offered so far, including the need for government's problem-solving attentions and the institution of ombudsmanlike units in some cities, which might make a difference in patterns of citizen contacting. However, as Coulter rightly acknowledges, none of the existing explanations seems to provide a satisfying answer to the puzzle of conflicting results.

Having set up the research context of an intriguing puzzle, the book moves to an original data analysis that the reader hopes will disentangle the inconsistencies and solve the puzzle. Most of the analysis is based on a sample of contacts received in 1983 by the Birmingham, Alabama Mayor's Office of Citizens Assistance (MOCA), a centralized complaints unit. The data analysis section is disappointing in some respects. For example, the possible role of neighborhood leaders as mobilizing agents is suggested elsewhere yet not pursued as a predictor variable despite the fact that some relevant data is available. In general, the scope of the data analysis effort in this slim volume is simply not substantial enough to tackle thoroughly the many possible solutions discussed here and elsewhere. But the data analysis section does produce a startling result—a heretofore unobserved relationship between social well-being and contacting propensity. The MOCA data show a curvilinear relationship between contacting and social well-being, *with middle-income neighborhoods generating the lowest volume of contacts.* Coulter also introduces data from a pair of survey-based data sets to suggest that contacters in Birmingham are prone towards other forms of political activity as well, thus demolishing notions that contacters are isolated parochials.

The final chapter of *Political Voice* is a discussion of the still-unresolved puzzle of conflicting findings concerning citizen contacting and socioeconomic status. It does not provide a comprehensive or totally compelling explanation of the puzzle, but it does offer an interesting set of observations that may provide some building blocks for a new theory. This chapter's best components are its discussion of the interaction between need for assistance and political awareness and its suggestion of a two-tiered conception of need. Still, despite these and many good suggestions about both theoretical and methodological weaknesses in existing research, Coulter does not provide a compelling synthesis to show how such disparate findings can have emerged from the study of citizen-initiated contacting. The puzzle remains, and future scholars may be motivated by Coulter's study to join in the hunt for a model that "covers" the many findings to date. This would be all to the good, because understanding individualistic demand making is surely a key to understanding political mobilization more generally.

ELAINE B. SHARP

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Dimensions of Hazardous Waste Politics and Policy. Edited by Charles E. Davis and James P. Lester. Westport, CT: Greenwood, 1988. 256p. $49.95.

This collection of articles, organized according to principal policy-making and implementation authority (i.e., local, state, national, international, and binational), addresses diverse issues related to hazardous waste management (HWM), including public participation, siting of HWM facilities, regulation, and enforcement. Collectively, they provide a status report (as current as possible, given publishing lag times) on these and other topics, with particular attention to policy evolution or change since the passage of the Resource Conservation and Recovery Act in 1976. Articles by Davis and Lester, Andrews, Bowman, and Barke on the domestic policy scene and Mangun on HWM policy in the EEC fulfill these functions rather well. It is a good, solid book that can be useful to many in the research and applied policy communities and is adaptable for courses addressing the policy process.

Although the jurisdictional framework is a convenient one, neither the editors nor the authors provide compelling theoretical or empirical reasons for such an approach—it is a loosely tied collection. Each article devotes some attention to introducing the reader to the policy arena, resulting in some collective redundancy. Relationships among the findings of the separate articles are not explored by the editors, except for a brief preface serving more to provide the reader a roadmap to the book than a sense of direction to the policy domain. There are other bothersome examples of editorial carelessness, such as missing and incomplete tables (in Zimmerman's article on HWM policy implementation in the context of empirical uncertainties) and even an unexplained table (in Grunbaum's article on judicial enforcement of liability for HWM). The otherwise comprehensive introduction provided by Davis and Lester—which identifies and summarizes research related to federal and state policy formulation, HWM facility siting, policy implementation, and findings from research in Europe and North America—is ultimately disappointing in its somewhat mechanistic approach to setting a research agenda (i.e., one based on the *number* of publications devoted to stages in the policy process and levels of analysis). While such a matrix may be useful for identifying absolute gaps in the literature, it fails to evaluate critically what research has been done and how any identified theoretical and methodological problems might usefully influence that agenda.

Some of the better articles are case studies. Matheny and Williams's piece on participation in HWM facility siting in Florida makes one wish that the state followed through with implementation of its "involvement" approach to allow a definitive evaluation of its utility. Kraft and Kraut's article developing the chronology of public participation in HWM facility siting in Clermont County, Ohio digs deep into the events and parties that affected policy formation and implementation. Mumme's article on binational negotiations between Mexico and the United States regarding water contamination challenges dependency theory nicely and validates and extends notions of complex interdependence to environmental policy. Rae Zimmerman's examination of the role of uncertainties in policy formation through a comparative case study of dredging decisions related to environmental contamination highlights one of the most compelling problems in the HWM policy process, as well as other science and technology policy problems.

The most ambitious article in the collection, perhaps, is Rowland, Lee, and Goetze's modeling of state HWM policy development. Acknowledging the partial success of previous research to explain variance in the stringency of state regulation of hazardous wastes, the authors enliven the debate by examining the dynamics of policy change in states using catastrophe theory. Although this approach has some intuitive and empirical appeal (it incorporates the nonlinearity of policy change), the application seems forced. Dependent and independent variables (really at a conceptual rather than an operational level) are manipulated through a maze of geometric variants of catastrophe theory (selecting the "butterfly" as the model of choice) to conclude that shifts from unresponsive to responsive (i.e., loose to stringent) regulation occurs when "unresponsiveness leads to continued net increases of waste deposition [*sic*]" in states (p. 112). Unless the reader feels compelled to follow the argument for its mathematical "elegance," he or she is likely to come away from the article confused at best, unconvinced at worst. As is the case with many such attempts, formal assumptions lead to questionable generalizations, which are fed into a jargon-filled approach, all of which get in the way of clarity of expression.

The price certainly inhibits broad adoption of this text for either classroom use or research. However, library adoption is recommended due to its occasional advancements in the fields of HWM policy, environmental policy, and the policy process.

SAM A. CARNES

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The Johnson Years. Vol. 2, Vietnam, the environment, and Science. Edited by Robert A. Divine. Lawrence: University of Kansas Press, 1987. 267p. $25.00.

The second volume of *The Johnson Years* provides an odd collection of essays on a variety of disparate subjects related to Lyndon Johnson and his administration. While the editor's attempt to cluster the papers in the book under three broad categories—"The Impact of Vietnam," "Protecting the Environment," and "Science and Public Policy"—is noble, the papers themselves suggest that the categorization oversells the content to follow and grants equal weight to analyses of unequal importance and substance.

Clearly, "The Impact of Vietnam" perhaps the most thorough section, does not respond to its billing. The chapter by DeBenedetti provides an interesting overview of the effect, or noneffect, of the Vietnam protesters on Lyndon Johnson's personality, psyche, and policies. Chapters by Donald Kettle and Burton Kaufman relate in summary form the effect of the war on fiscal, monetary, and foreign aid policies. Lacking in all the chapters are specifics concerning opportunity-costing equations and political, social and economic variables that led to specific strategies to cut certain domestic programs and not others or to raise certain kinds of taxes and not others. Common to all chapters is the pervasive and, indeed, important theme concerning the president's inability to make a clearcut choice between guns and butter, between options suggested by his military advisors to win the war in Vietnam and options proposed by his domestic aides to, assumedly, win the peace in the then tense, often burning, ghettos. Indeed, most of the authors suggest that the president purposely and consciously chose to walk a narrow line between the advocates of an all-out war in Vietnam, which he felt we couldn't win, and an all-out war on poverty, which he felt he couldn't wage given budget constraints. He didn't want to lose the war in Vietnam, and he didn't want to give up the Great Society. His was truly, according to the authors, a minimax dilemma.

The "truth in advertising" is again a problem in the section titled "Science and Public Policy." It contains two chapters, "The War on Disease" and "Lyndon Johnson and the Politics of Space." Regrettably, given again the editor's and authors' nomenclature, "War on Disease" focuses primarily on the war between proponents of basic and applied medical research. Nowhere in the chapter do we receive information or analysis on the significant battles in the Johnson administration over the level of scientific research and the role of university research. More importantly, perhaps, do we secure analysis concerning the initiation of Medicare or Medicaid, certainly two of the milestones in this nation's war on disease? In this context, the authors' acknowledgment that both programs are covered in other books by other authors provides only a partial nostrum. The reader was promised more.

One more bothersome and basic problem with the book exists. The editor at times is overly charitable or perhaps unable to make even gross distinctions regarding the appropriateness and depth accorded each chapter. Lyndon Johnson's administration may well have provided the transition between the limited environmental agenda implicit in the conversation movement and the ostensible effort to forge a more vital environmental policy in the Nixon administration. But the chapter by Melosi, "Lyndon Johnson and Environmental Policy," doesn't prove the point. What Melosi describes is a collage of disparate efforts—some of which succeeded and some of which failed—to secure legislation or reorganization relevant to the management of public lands, water, wildlife, and the urban environment. He also portrays the influence of key people inside and outside the Johnson administration in framing environmental issues for the president. But the sum of the parts does not add up to a cohesive environmental policy. Clearly, the addition of the chapter by Lewis Gould on Lady Bird Johnson and beautification does not help. It is interesting to read but not directly related to the evolution of the environmental movement. The effort to link what was Mrs. Johnson's positive effort to improve the physical appearance of Washington and other areas of the nation with Johnson and his advisors' assumedly evolving interest in more complex environmental problems fails. Lady Bird Johnson's activities were, and should be, applauded. But as the author himself indicates directly and by inference, they were limited both in concept and in practice. Their ability to shape the character of the president and his advisors' interest in broader environmental issues and their capacity to become a key variable—like Rachel Carson's *Silent Spring*—in generating the environmental movement was marginal.

While the book claims more than it delivers, many of the individual papers in it offer more than most books of a similar venue. By and large, they are well written and amply footnoted and provide interesting, sometimes heretofore unpublished material relative to the Johnson administration. Generally, all the papers view the president as a complex, vigorous, and powerful leader. Contrary to most of the current conventional wisdom, they also see him as an individual deeply committed to, and even passionate about, responding to the needs of the nation's poor and sick, as well as to the needs of the nation's fragile environment. All the authors agree that Johnson was a politician and a master of the political deal. But he had a bottom line. While it generally included his own political well-being, it also included what even his critics would define as the public good or the public interest. Clearly, the authors do not feel that their collective picture of Johnson fits the inherently flawed, amoral Johnson in Caro's *Path to Power* or the demonic Johnson in Richard Goodwin's *Remembering America: A Voice from the Sixties.* In this context the authors present a picture of Johnson that is more sympathetic than the earlier literature on the president. Perhaps the essays will help pave the way for a balanced and studied portrayal of the Johnson era and personality.

Interestingly, each of the articles, apart from the unanswered guns-and-butter equation, suggests three key factors associated with decision making in the Johnson administration: lack of focus, unstable analysis, and reliance on people over structure.

Most of Johnson's experts failed to provide him with a focused set of policy objectives or ideology within which decisions concerning the Great Society programs or, indeed, the Vietnam War could be made. Johnson's economic advisors, his military leaders, his environmental advocates, and his health staff were competent, sometimes brilliant. They were committed individuals. But they were an eclectic bunch. No one person apparently dominated. Many individuals had access to the president. Consistency and predictability regarding vision were rare characteristics. Technicians became politicians, and politicians technicians.

Put two economists, health specialists, environmentalists, and military experts in a room with the president and you generally got three, four, or more answers with respect to the problem or policy option on the agenda. Clearly, the analytical environment was unstable and unpredictable. Just as clearly, given the wide variation in the numbers produced by the experts—whether the issue was the budget, Vietnam, or space—methodological precision was rare. Lindblom's muddling through typified the behavior of the staff—even the best and the brightest among them—as well as of the politically elected leaders.

Structure and structural reform—both on the table, particularly in the health, space, and environmental areas of concern—were not major considerations either with respect to policy options or to policy development. Mapping the relationship between people and structure in terms of their respective influence on policy is difficult in light of the brevity of most of the chapters in the book. But if "winners" had to be identified, people were the most important variable determining Johnson and his administration's actions. Without Mary Lasker, philanthropist and business leader, Mrs. Johnson's beautification activities and the president's health policies would have suffered. Without Jim Webb, the Apollo mission to the moon would likely have been delayed or never even have occurred—and so on. Certainly we should know more about the effect of individuals on structure and vice versa. The book, however, appears to confirm other recent literature on policy making that lends credence to the hypothesis that "people can surmount structure."

MARSHALL KAPLAN

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Poor Support. By David T. Ellwood. New York: Basic Books, 1988. 271p. $19.95.

Responsibility, Rights, and Welfare. Edited by J. Donald Moon. Boulder, CO: Westview, 1988. 235p. $38.50 cloth, $16.95 paper.

America's Hidden Success. By John E. Schwarz. Rev. New York: Norton, 1988. 269p. $6.95 paper.

These volumes are timely and provide solid contributions to our understandings of welfare, democratic theory, and U.S. politics. In particular, they help structure our thinking about three major questions surrounding welfare policy. First, is there a right to welfare and, if so, what is the proper role of contemporary governments in providing it? Second, why is the public, particularly since 1970, so disaffected with welfare? Third, how can we organize more efficiently and effectively to mediate problems addressed by welfare policies? Read together, these books provide a useful background to begin answering these questions.

Questions surrounding welfare rights and appropriate government roles have long concerned students of politics. In each of these books, most notably the Moon volume, the questions are addressed in light of liberal political theory, which has provided much of the justification for Reagan-era social welfare policy. Its contributors attempt to locate the proper role of government in the provision of income and other social support, relying largely upon liberal theory and notions of moral obligation. Classical liberalism, argue several of the contributors, provides justification for the welfare state, contradicting the interpretations of several contemporary liberal thinkers and calling into question the theoretical basis for much of the contemporary U.S. federal welfare program. As Stephen Holmes argues in his essay, nineteenth-century liberalism was a broadly based line of thought open to widely divergent interpretations in numerous areas, including the role of the state in the provision of welfare and the rights and obligations of citizens in redistribution. Essays in this volume support the notion that the role of redistribution in liberalism has often been misconstrued. Arguments for the welfare state take two major approaches: in the first, it is argued that liberal theories provide justification for welfare provision as a necessary precondition of a market economy. In the second, the provision of welfare is presented as a social duty based on moral principle.

John Schwarz's *America's Hidden Success* is concerned less with questions of welfare rights and more with the government role question. The central thesis of his text is that positive government in the United States has worked and that it is our best hope for improving the welfare problem, as well as other public problems. Consistent with arguments in the Moon volume, he contends that the widely trumpeted new public ideology embracing tenets of classical liberalism (e.g., individual freedom, self-reliance, and minor governmental intervention) rests on the false premise that positive government has failed. He demonstrates—through a mixture of (admittedly) Reaganesque vignettes highlighting individual cases and discussions of aggregate data—that U.S. social policies since the first Eisenhower administration have made a difference: poverty is lessened, low-income persons are more likely to receive needed medical care, infant mortality is down, people are more apt to be fed, and so on. Schwarz contends that these improvements are functions of welfare and other positive government policies—not purely of economic growth, as conservative observers contend. Further, he argues that much economic growth is the product of positive government action. Ellwood, like Schwarz, dismisses conservative arguments that welfare should be focused in some area other than the state (e.g., the private sector), but is more critical of governmental performance and concerned with addressing problems in the system.

Public disaffection with government generally and welfare specifically is portrayed as a function of the public's failure to grasp the changing nature of public problems, of the nature of welfare, and of development. Schwarz portrays the 1970s as giving rise to a series of new problems: stagflation, environmental pollution, changes in the labor market brought about by technological advances and the coming of age of the baby boom generation, and so on. The problems were new, Schwarz argues, but probusiness and other conservative forces were successful in promulgating the belief that the problems were caused by the government. Conservative success in getting out the message, along with a pronounced public appetite for free lunches, contributed to the rise of a false image of governmental failure. Addressing the welfare question more directly, Ellwood argues that we dislike welfare for more fundamental reasons: it brings into conflict some of our precious public and personal values. For example, we want to take care of those unable to care for themselves, but we also value individual initiative and autonomy. Claus Offe, in Moon's volume, suggests that welfare is less controversial in homogeneous societies, as citizens are more inclined to want to share with persons of similar characteristics, backgrounds, and the like. As societies become more heterogeneous, redistribution presents greater difficulty.

Of these books, David Ellwood's *Poor Support* offers the most cogent critique of a portion of the welfare system and provides the most useful suggestions for future policy. He shows that family support in the U.S. provides strong inducements for the family to disintegrate, despite the popularity of the family in contemporary political rhetoric. His analysis concerns three large family groups in the poverty population: the working poor, which includes both one- and two-parent families that remain in poverty even though one or both parents are working, making them ineligible for various public assistance programs; families who are temporarily in poverty because of job loss or other changes in their situation; and families headed by chronically unemployed adults. The present welfare system fails to distinguish the different types of poverty among families, he argues, allowing economic and social factors, along with the system's design, to combine to make our welfare system dysfunctional and to keep people living in poverty. A more realistic welfare system would account for the different types of poverty and be designed to mediate particularized problems. The goal of the system should be to enable poor people to leave the system; so simplistic solutions (e.g., the liberals' increasing the payments and the conservative's removing people from the roles) will not work. Instead, Ellwood argues, the difficult task of restructuring the system is necessary.

The consistent message of these volumes is that—rhetoric to the contrary—considerable economic and moral justification exist for a state role in the provision of welfare support. Essays in Moon's volume address this and related questions in considerable detail and help provide a framework for understanding contemporary welfare policy. Schwarz's book is most useful in its analysis of public attitudes toward government and welfare and for that reason may not fairly be evaluated as a contribution to hard policy analysis. His analysis of policy performance is, by definition, evaluative but fails to address the contradictions of welfare policy performance as cogently as Ellwood, whose book may prove to be a classic study of poverty in the United States and is deserving of considerable attention among policy makers.

To conclude, each of these books is worthwhile reading for students of welfare policy and U.S. politics. They do not answer all the questions surrounding this difficult policy area, nor do they set out to do so. The role of the states is given little attention in any of these books. Schwarz and Ellwood, both of whom write specifically about U.S. welfare policies, fall soundly on the side of nationalization of U.S. redistributive policy with only passing consideration of state roles in its provision and administration. Any reform of welfare policy must account more thoroughly for state roles and the politics of reforming so massive a transfers system.

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Leadership in the Modern Presidency. Edited by Fred I. Greenstein. Cambridge: Harvard University Press, 1988. 419p. $29.95.

Fred I. Greenstein has edited papers presented at the Conference on Leadership in the Modern Presidency, held at Princeton in 1987, to make up this important new book on leadership in the modern presidency. William E. Leuchtenburg writes about Franklin Roosevelt as the first modern president. Readers familiar with Leuchtenburg's work will be amazed at how the author contributes new anecdotes and examples to back up his analysis and prove that everything has not been written about FDR yet. Alonzo Hamby continues the trend toward Truman revisionism, and Greenstein argues that Eisenhower was not only a hidden hand leader but a leadership theorist. Carl Brauer writes about the endurance of inspirational leadership with respect to John

Kennedy's presidential performance, and the ever-clear Larry Berman indicates in his article some of the paths chosen and opportunities lost for the Lyndon Johnson presidency. Joan Hoff-Wilson details the Nixon administration's attempt at the corporate presidency, and Roger B. Porter contributes his analysis of Ford's leadership abilities in his healing presidency. Erwin C. Hargrove evaluates Jimmy Carter's presidency in terms of the politics of public goods, and William K. Muir maintains that Ronald Reagan's style can best be understood in the primacy of rhetoric. Greenstein concludes the volume with an essay, "Nine Presidents: In Search of a Modern Presidency."

Presidential leadership is a complex subject. For some writers presidential leadership means that a president has the ability to lead the White House staff. Other writers concentrate on the ability of a president to lead the entrenched bureaucracy to do things that it would not normally do. Still other writers concentrate on the president's ability to lead Congress, the special interests, public opinion, and the Western alliance. Leadership ability then is a matter of perceptions. It is clear that to be a leader one must have followers and supporters. The ability to provide presidential leadership is evaluated in presidential performance. It is a valenced concept as evaluated by presidential watchers, media experts, political scientists, historians, public opinion polls, and citizens, against the backdrop of expectations and predispositions of the evaluators.

I am particularly impressed by the Leuchtenburg article on FDR and the Hoff-Wilson evaluation of Nixon's leadership. Hoff-Wilson does her best to consider Nixon's record on the substantive achievements and failures of his presidency without concentrating on Watergate in her analysis. In effect, she asks the reader to consider Nixon without Watergate. This radical proposition allows the author to examine Nixon's record in foreign relations, domestic reform, and administrative reform of the modern U.S. presidency.

Brauer does a competent job in showing why JFK's inspirational leadership style continues to draw rave reviews in the public opinion poll. However, the author probably should have mentioned the assassination plots against foreign heads of state planned by members of Kennedy's administration and some of JFK's extramarital affairs with Judith Campbell Exner to be fair to some of the critics of Kennedy's performance. On balance, he does indicate the success of an enduring Kennedy leadership style for some citizens.

Roger Porter was able to write once again as an authoritative insider for the Ford administration, but Porter's research effort revealed once again that presidential scholars have not written very much about the Ford presidency. Most of Porter's research comes from Ford's book, Porter's book, Kissinger's views, and inside accounts and interviews. Sooner or later, presidential scholars will have to move beyond the inside accounts of the Ford years and treat those times like other presidencies.

If Larry Berman had followed Hoff-Wilson's approach, he would have done Johnson without Vietnam. Berman did not and should not have done this. As he has done in other works, Berman points out some of the tragic elements in Johnson's leadership style. He indicates, as other authors have, how the best-laid plans of U.S. presidents can fail to such a degree that it becomes the legacy of their administration.

Erwin Hargrove's evaluation of Carter is not about to touch off any new wave of historical revisionism to argue that "Carter was not that bad," yet Hargrove does argue that Carter had some strengths as a leader and many deficiencies in leadership style that cost him considerable support. Finally in the most controversial article, Muir maintains that Reagan's rhetorical skills are what returned presidential leadership in the 1980s to new-found stature. Muir argues that Reagan's "moral revolution" has succeeded in the United States. He portrays Reagan as an outstanding public educator who used the pulpit of the presidency to lecture citizens about equality, freedom, and U.S. morality. Muir obviously hasn't been observing the same presidency I have. Alternative views of the Reagan presidency would ask the reader to consider the creed of "greed" over caring, the Iran-Contra affair, and the so called "sleaze" factor of the Reagan administration. Muir is silent on these issues. Presidents also teach by their action and inaction rather than instructing by their words.

Finally, Greenstein attempts to make sense of the book and the modern presidency in his concluding essay. He is remarkably successful in sorting out influences and developments in the modern presidency. For Greenstein, FDR provided the breakthrough for the modern presidency and Truman provided the institutionalization of it. Eisenhower's years ratified the modern presidency and Kennedy provided the beginnings of the "later modern presidency." Johnson was important for his executive role and his legislative style, while Nixon is crucial for understanding the administrative presidency. Ford provided post-Watergate leadership and Carter tried to provide Washington with his outsider style of leadership. Greenstein then assesses the Reagan revolution and on balance finds that Reagan restored strong presidential leadership to the political system.

This volume is highly readable and an important scholarly contribution to the study of the U.S. presidency. In one volume it provides readers with an excellent overview of the evaluations of presidents from FDR to RWR, and it provides a quality review of the institution of the modern presidency.

JOHN ORMAN

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The Presidential Branch. By John Hart. New York: Pergamon, 1987. 250p. $32.50.

While presidential aides were once believed to need a "passion for anonymity," in our time the chief requirement for a White House job seems to be a good literary agent. Former aides often write books about the presidents they served. Frequently these are "kiss and sell" memoirs that succeed in the marketplace to the extent that they reveal juicy anecdotes about powerful people. But the presence of such gossip on the best-seller lists obscures the actual importance of presidential staff, because the vast majority of aides are anonymous and never come under indictment.

The truth about the president's staff is that it has become a sort of "general secretariat" through which the chief executive attempts to assert his influence in U.S. government. The executive office makes the president an autonomous actor in contemporary government. Nancy's astrologer and Lyndon's personal habits may seem more newsworthy, but they pale into insignificance next to the often-unreported work that goes on in the "presidential branch." Only when things go wrong, as they did in the Iran-Contra affair, does the media give the public a view of the inner workings of the president's staff.

The Presidential Branch is a valuable antidote to this lopsided coverage of the executive office. Hart has provided an excellent survey of the presidential staff and its relevance to U.S. politics. In a relatively short volume, Hart describes the origins and growth of the executive office, its structure and operations, the relationship between presidential staff and Congress, and the debate over the future of the "presidential branch." The book's brevity makes it all the more useful, for it is accessible to experts and nonspecialists alike.

Scholarly interest in presidential staff is now in its third phase. The earliest works in this area focused on individual aides, and the best of these studies (e.g., George and George's *Woodrow Wilson and Colonel House* [1956] or Patrick Anderson's *President's Men* [1968] still provide important insights into the workings of the presidency. The second generation called attention to the growth in the size and power of the executive office, giving us such concepts as the "institutionalized presidency," Thomas Cronin's notion of the "swelling of the presidency," and Nelson Polsby's term, *the presidential branch*. Hart's book is representative of the best of the third generation of works on presidential staff. It offers a natural history of the executive office, as well as a description and analysis of how this "presidential branch" fits into the U.S. system of government. Along with other third-generation studies of merit (including Bradley Patterson's *Ring of Power* and James Pfiffner's *Strategic Presidency* [both 1988], *The Presidential Branch* is an important book for anyone who wants to understand the White House in our times.

If the book has a flaw, it is Hart's underestimation of the size of the "presidential branch." Bradley Patterson, a veteran of three administrations, puts the number of presidential staff at well over three thousand and presents a convincing case for his estimate. Hart's estimate, while larger than most previous ones, misses Patterson's authoritative numbers by about 50%. Of course, much of the reason for this discrepancy lies in the secrecy surrounding the executive office, with actual personnel figures either hidden from view or buried in the accounts of several government departments. Nevertheless, Hart's readers ought to be aware of this problem.

I have argued in other places that the contemporary presidency is "postmodern" in nature, in part because of the roles and functions that the executive office assumed in the past two decades. What I call the president's "general secretariat" is the same creature that Hart and Polsby call the "presidential branch": a complex, bureaucratic, and politically powerful presidential staff. No matter that many political scientists and other observers lament the rise of this staff as a force in U.S. government. It will not disappear or change significantly in the near future. Future presidents, regardless of their party or ideology, will continue to rely on it. Hart's book helps us to understand why that is so.

RYAN J. BARILLEAUX

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Managing the Presidency: The Eisenhower Legacy—From Kennedy to Reagan. By Phillip G. Henderson. Boulder, CO: Westview, 1988. 205p. $29.95.

From the 1950s through the 1970s, the Eisenhower administration was thought to offer scholars of the presidency useful lessons in how not to exercise leadership effectively. Seminal articles and books by both journalists and academic observers—Richard Neustadt most prominently—portrayed a presidency mired in government by committee, excessive delegation of authority, passivity in congressional relations, and a general inability to transfer Eisenhower's popular appeal into an effective political program. Even Eisenhower's presumed strong suit—skill and organization in the making of foreign policy—not only came under academic fire but also was roundly criticized in 1960 by Senator Henry Jackson's Subcommittee on National Policy Machinery.

Following declassification in the late 1970s of a large amount of material in the Eisenhower Library (especially the files of Eisenhower's personal secretary, Ann Whitman), scholars have discovered a much different Eisenhower presidency: a chief executive who combined formal and informal modes of decision making, delegated authority selectively while always retaining the ultimate power to decide, and attempted to exercise influence in his dealings with Congress in indirect, nonconfrontational ways. In short, as Greenstein has aptly suggested in his seminal study, Eisenhower exercised "hidden hand" leadership techniques, all the while portraying himself to the press and public as above the political fray.

Henderson's study lies squarely within this emerging Eisenhower revisionism. The first chapter offers an excellent review of the developing body of empirical research on the Eisenhower presidency. The bulk of the chapters provide a well-written and well-documented analysis of key features of Eisenhower's staffing structure, his institutional presidency more broadly conceived, his mode of decision making, relations between the White House and cabinet, and his dealings with Congress. Students of foreign policy decision making will find his lengthy discussion of Eisenhower's national security machinery especially of interest. Throughout the work, Henderson takes pains to relate Eisenhower's experiences to those of his successors in office, who usually fall short of Ike's mark. Chapter 7 offers especially timely reflections on the experience of his most recent successor, who shares Eisenhower's popular appeal but whose managerial style has led to policy fiascoes like the Iran-Contra affair.

Several weaknesses, however, beset Henderson's project. Unfortunately, there is not much here that will strike scholars of the Eisenhower presidency as new or revealing. The Eisenhower that Henderson depicts is essentially the Eisenhower that has been well-explored in Ambrose, Divine, Ferrell, Greenstein, Reichard, and Kinnard. What is needed at this stage in Eisenhower revisionism is not further testimony of Ike's behind-the-scenes prowess but more in-depth examination of how his leadership style and mode of decision making operated in particular instances and whether it actually had a positive impact on policy outcomes. Henderson discusses the Formosa Straits crisis, but other cases are probably more telling about such critical questions as, Did Eisenhower's decision process matter (Indochina in 1954)? Did it always operate to produce optimal policy choices (the overthrow of Mossadegh or Arbenz)?

Given Henderson's interest in comparing the Eisenhower institutional presidency to those of his successors, another important question that might have been addressed is precisely what accounts for the success that Henderson and others attribute to the Eisenhower presidency. Nixon's reliance on formal decision-making bodies and procedures was similar to Eisenhower's. Does this suggest that Eisenhower's own personality and leadership skills are the critical set of variables? Kennedy and Johnson deinstitutionalized much of this formal structure and process. Does this suggest that it is organizational level variables that matter? That such critical issues are suggested by Henderson's work is its strength. That they are not directly addressed is its weakness.

JOHN P. BURKE

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Reforming Defense: The State of American Civil-Military Relations. By David C. Hendrickson. Baltimore: Johns Hopkins University Press, 1988. 152p. $24.50.

When the Korean War started, relations between the Defense and State departments were at an impasse. Louis Johnson, the secretary of defense, had been insisting for nearly a year that all dealings between the two departments be handled through his office—indeed, through him. The rule could scarcely be taken seriously, and when Harry Truman fired Johnson and appointed George Marshall in his place, Marshall and Dean Acheson, the secretary of state, quickly acted to strengthen working relationships between State and Defense. Among other things, as it is told, they agreed between themselves personally to throw out of any meeting they attended any official who began a statement with "Well, from a military viewpoint" or "Well, from a political [as distinct from a military] viewpoint." No one counted how often the two men acted upon this commitment, but the story tells clearly of their viewpoint: those who attempt to partition foreign and defense affairs into political and military aspects are suspect, or worse.

David Hendrickson, in a superbly written little book, *Reforming Defense,* takes a different view. He attempts to bring order and proportion to the defense reform activities of recent years in the United States by distinguishing between civilian and military functions in the U.S. government. It is a challenging task. No one has ever done it well. The author's effort is among the best.

Hendrickson's book is addressed to the corpus of recent defense reform literature. Its conclusion about the reformers distinguishes between the reorganizers and the reformers. The former, who are largely responsible for the Goldwater-Nichols Act of 1986 and the Packard reforms of that year, he contends, addressed the civil-military division and dealt with it constructively. The latter, he argues, ignored it, with serious consequences. Their recommendations would weaken, not strengthen, the military establishment.

The debate over defense reorganization often lacks depth. Hendrickson's elegant essay helps correct this weakness by addressing organization theory without apology. He has taken military professionalism seriously, treating it as a valuable asset, as he should. He also correctly sees the armed services as institutional assets. It follows that he should take the division of labor between civilian leaders and the military seriously; and he does, discovering in this line of logic the utility of Samuel P. Huntington's concept of "objective" civilian control, which sharply distinguishes things civil from things military.

But Hendrickson does not make fully persuasive—nor did Huntington—the case for strictly segregating civil and military tasks in a U.S. government where, as Richard E. Neustadt observed, power is not separated but shared. He has failed to explore the complicated interdependency of these tasks. For instance, he marks Eisenhower's unwillingness to entertain "split papers" from the Joint Chiefs of Staff (JCS) as a cause of heightened interservice rivalry but fails to test this claim against a common observation about the experience of the Kennedy administration, that the JCS avoided split papers—avoided interservice conflict in the JCS—as a protective measure against an aggressive secretary of defense, Robert McNamara.

Similarly, his handling of criticisms of "servicism" (i.e., the narrow pursuit of service interest) and of the Joint Chiefs of Staff, while concise and fair, do not take into account the underlying concerns for civil-military relationships that have driven some of them. He claims that servicism has been blamed too often for what ails defense policy and organization. In this connection, he charges that complaints about the recent working of the JCS system are misplaced. It is not the excessive pursuit of service interests, he argues, but the policies of the Reagan administration that must be held responsible for "strategic pluralism run amok" (p. 44). This charge is fair enough as far as it goes, but it does not dispose of the complaint that the JCS has encouraged servicism at the expense of coherent professional military judgments that could facilitate the making of sounder civilian decisions. Two major proponents of defense reform, Generals David C. Jones and Edward C. Myer, seemed to have this in mind.

Normative organizational studies usually encounter difficulties in attributing sufficient weight to factors that lie beyond the practical reach of organizational design. *Reforming Defense* shares this difficulty with Huntington's seminal work, *The Soldier and the State.* "The great object in the design of higher defense organization," Hendrickson writes, should be to ensure a plurality of military advice on those questions that involve an intermingling of political and military responsibilities" (p. 115). He is wrong. The great object of *government* is to assure this sort of pluralism, and in this respect the U.S. government has been successful. Defense organizational reform can proceed with the confidence that it will not substantially affect the plurality of military advice that is available to the president, the Congress, and the public. Taking this pluralism of government into account should change the direction of inquiry about the norms of defense reform even for those who are serious about civil-military distinctions. The question becomes not how to assure that the president has available varied military viewpoints but how much effort should be devoted in statute, administrative arrangement, and presidential practice to seeing that at least one military leader attempts to speak for a presidential viewpoint.

Hendrickson recognizes the importance of technical innovation to the U.S. armed forces and attributes responsibility for it to the military professionals. The U.S. armed forces rely on superior technical quality in their arms because they cannot compete in quantity. Innovation is, therefore, a critical dimension of their performance. Hendrickson defines the question about innovation as whether to delegate the management of technology to the officer corps or not. It is the wrong question. Innovative organizations are not manned by worried turf defenders. Hendrickson himself writes approvingly of William S. Sims, whose reforms the navy surely needed, and who would never have succeeded without outside support. There is in Hendrickson's protective concern for the services a justifiable antidote against the contempt of instant outside experts. But his concern goes too far. Since the sixties business, health, law, regulatory government, and police must all face more challenge from outsiders, some of them well-informed, some not. The military is not alone in being subject to this sort of intrusion nor particularly disadvantaged in coping with it.

This book should serve several kinds of readers: the military expert, the military activist, the student of military affairs and bureaucracy, and the professional officer corps. It belongs on a short list for the latter.

PAUL Y. HAMMOND

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Government by Proxy: (Mis?)Managing Federal Programs. By Donald F. Kettl. Washington: Congressional Quarterly, 1988. 170p. $10.95 paper.

"Third-party government"—where government seeks to accomplish its goals by making arrangements with other governments, the private sector, and private nonprofit organizations to carry out its programs—is as old as the republic. But since the 1960s third-party methods have become increasingly common, and discussion within public administration of the management problems they cause is long overdue. In fall 1987 the National Association of Schools of Public Affairs and Administration—the accrediting body for Master of Public Administration programs—joined the debate by sending its member programs a copy of a 1986 speech by Lester Salamon to the National Academy of Public Administration. Salamon suggested that the phenomenon posed significant problems for the normal practice of public management, the paradigms of which assume that government is implementing its *own* programs through hierarchical organizations.

Now Donald F. Kettl has written a book-length analysis of government by proxy, pointing out its significance as government attempts to carry out its objectives amidst budget and personal constraints that make proxies extremely attractive as implementing organizations.

Kettl investigates five programs by analyzing two examples in each, one that worked poorly and one that worked well. For government contracts, the Division Air Defense gun, the *DIVAD*, is an example of a poorly managed project cancelled after the expenditure of $1.8 billion. In contrast, the *VIABLE* computer project, in which the army replaced its entire worldwide computer network, was managed on time and under budget. The difference, Kettl maintains, is in how the contracts are "managed." The goal should be to keep to the overall, long-term aims of a project and not to subordinate those goals to the details of project procedures and specifications.

The next chapter analyzes the community development block grant program's implementation in Bridgeport, Connecticut, contrasting its travails with the "negotiated investment strategy" (NIS) used in the same state to decide how massive cuts in the state's social services budget would be implemented. The NIS was essentially a strategy in which the interested parties both inside and outside government came together to negotiate the exact cuts prior to their announcement and implementation by government. The NIS strategy shows that the conflicts over goals inherent in government by proxy can be overcome. But any NIS requires interested parties (in this case both state government and private foundations) willing to support that strategy with time, money, and attention.

A chapter on tax expenditures investigates the whole range of incentives given through the tax code to individuals and corporations in an effort to induce them to behave in accordance with government policy without direct expenditures. While this chapter fits within the conception of the book, it does not contribute as much as the others to our knowledge of the trade-offs involved in government by proxy.

The chapter on loans analyzes the federal government's student loan programs in depth to show how the shift from grants to loans under the recent administrations has changed the nature of the administration of these programs. One implication of this shift has been to make it more difficult to collect on defaulted loans. Another has been to shift the incentives for the states, some of which have begun their own programs targeted at the better-off students, leaving the poorer students (who are less able to repay their loans) for the federal government.

A chapter on regulation discusses the Arrow Air disaster in Newfoundland to introduce a discussion of regulatory strategies. Traditional regulation and the Reagan era's excesses of de- or nonregulation are contrasted with the EPA's "negotiated rule making" idea, under which the involved organizations are invited in to the agency before rule making takes place in order to work out problems prior to the stage when legal action would be an available strategy.

The bottom line is that value differences over program goals permeate all of these different proxy government strategies. Kettl, like many recent implementation researchers, asks whether failure is inevitable. His answer, however, is different: he feels that the standard procedures of control used in hierarchical organizations—regulation, centralization, auditing, collecting better information—are simply inappropriate and add new problems and a new layer of bureaucracy for program managers. Instead, he suggests that careful management of proxy programs—management geared to the needs of each individual program—can lead to effective government. He suggests that the successful examples in each chapter provide us with models of that success. The successful examples are much more limited than the traditional examples of failure, however; several of the successful models require extraordinary amounts of time, effort, people, and commitment to consensus decision making.

At the center of *Government by Proxy* is a fundamental assumption that becomes increasingly explicit as the book proceeds: the "management problems" that permeate government by proxy are just that—problems of *management* that have a political origin but are solvable through a management style different from that used in traditional hierarchy. While I would personally place more emphasis on fundamental political differences as the origin of these disputes, Kettl's viewpoint is a legitimate one well worth pursuing in this era of limited resources. This is an important book for students of implementation and public administration.

J. THEODORE ANAGNOSON

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Working but Poor—America's Contradiction. Sar A. Levitan and Isaac Shapiro. Baltimore: Johns Hopkins University Press, 1987. 142p. $22.50 cloth, $7.95 paper.

The recent debate over the job training and work fare provisions of welfare reform has concealed a fact many find startling: most poor families contain members who already work, and much of the income of poor families, especially poor families with two parents, comes from wages rather than welfare. The working poor represent an uncomfortable anomaly for U.S. citizens who view work as a way out of poverty and believe that no family with a full-time worker ought to be poor. In 1985, some six million poor individuals lived in families in which someone had a full-time, year-round job, and yet the total income of each family was below the spartan (e.g., $13,000 per year for a family of four) Federal poverty standard. *Working but Poor,* by Sar Levitan and Isaac Shapiro, is an accessible volume describing the working poor and detailing current and recommended policies to assist them.

The authors identify a number of reasons why work and poverty can still go hand in hand in the United States in the 1980s: stubbornly high unemployment rates; jobs lost to technology, imports, and immigration; baby boom labor supply bulges; a reduced federal commitment to training and public job creation; and the falling real value of the minimum wage. The authors present a refreshingly evenhanded assessment of these issues, emphasizing that technological change, foreign trade, and immigration lead to benefits as well as costs and do not merit the political demagoguery often currently directed toward them. Most of the items on the list of reasons have contributed to the problem of working poverty. Taken together, however, this list fails to account for the dramatic increase over the last two decades in the inequality in the wage distribution that lies at the core of the problem of low-paying jobs.

The authors view the fact that inflation has so eroded the minimum wage (its purchasing power was no higher in 1985 than in 1955) as both an important cause of working poverty and as the starting point for their list of recommended policies. Critics of an increase in the minimum wage often allege that such a step would trigger the loss of many low-wage jobs. However, the fact that unemployment problems have persisted over the last decade despite a substantial drop in the inflation-adjusted minimum wage, along with more careful econometric assessments, suggests that employment effects are likely to be rather small. The central problem with minimum wage policy changes is that they are so inefficient: only a minority of minimum wage workers live in poor families. (Poor families contain either no or low-wage workers, but many low-wage workers are secondary earners in not-so-low-income families).

Targeting is achieved much more effectively through income tax policies such as the Earned Income Tax Credit (EITC), which rebates to workers in low-income families a portion of their earned income. Indeed, the expansion of the EITC and other reforms in the income tax legislation passed in 1986 have done a great deal to remedy the appalling jump in taxes paid by the working poor in the 1970s and early 1980s. The authors argue against further reforms in the income tax law and in favor of raising the minimum wage on the grounds that the latter helps nonpoor families, doesn't increase the federal deficit, provides workers with more earned income rather than a government transfer, and is paid throughout the year instead of once annually. Readers who attach more weight than the authors to the virtues of targeting and the vices of the modest disemployment effects and hidden costs of price increases passed on to consumers will probably continue to view tax law changes as the most promising approach to helping the working poor.

Prominent among the $10-billion set of policy changes recommended by the authors are a number of government training and public employment measures, many of which have been cut by the Reagan budget knife. The authors provide a rosier-than-conventional-wisdom assessment of job creation programs such as the Comprehensive Employment and Training Act, the Targeted Jobs Tax Credit, and the Jobs Training Partnership Training Act. Here again, readers less enamoured with the record of public employment programs will beg to differ.

Evidence on the effects of several government training programs is somewhat more encouraging. Experimental evidence from several states indicates measurable, if not spectacular, success in enhancing the employment prospects of long-term welfare recipients. Programs such as the Job Corps and Head Start have also received positive evaluations. However, it is important to remember that "success" for these programs often means that someone formerly dependent on welfare joins the ranks of the working poor. The problem of workers who are poor is as much a function of low rates of pay as lack of work.

Occasional disagreements over the book's recommendations should not be taken to indicate that it has fundamental flaws. On the contrary, it is a uniformly accessible, informative, and fair treatment of its subject. The task of placing perspective on the myriad issues bearing on the problem of workers in poverty is a daunting one, yet the book's discussion of each issue reflects knowledge of current research findings and presents a balanced discussion of the costs and benefits of policy options. It offers a solid introduction to the problems, prospects, and policies surrounding the working poor.

GREG J. DUNCAN

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The Dilemma of Toxic Substance Regulation: How Overregulation Causes Underregulation at OSHA. By John M. Mendeloff. Cambridge: MIT Press, 1988. 321p. $35.00.

Most critiques of government regulation over the last decade have argued that we in the United States suffer from overregulation. Also, most critiques of government regulation over the last decade have been based on some form of cost-benefit analysis. As a result, many proponents of regulation have come to distrust cost-benefit analysis and assume that it inevitably lends support to deregulation. This study shows otherwise.

John M. Mendeloff's excellent analysis of the U.S. Occupational and Safety and Health Administration (OSHA)'s standards for toxic chemical levels in the work place builds a case for *decreasing the severity* of exposure limits in order to *expand* standard-setting activity *to a much larger number* of toxic chemicals.

Mendeloff argues convincingly for the paradoxical proposition that OSHA's overregulation of a few toxic chemicals has stymied attempts to regulate a broader array of dangerous chemicals, thereby producing underregulation of the vast bulk of hazardous chemicals. *Overregulation* and *underregulation* are defined in cost-benefit terms: with overregulation, marginal costs exceed marginal benefits; while with underregulation, marginal costs fall below marginal benefits. Overregulation occurs when standards are so strict that implementation costs far exceed the health protection benefits generated by them. This stimulates strong employer resistance to implementation of existing standards and stiff political opposition to regulation of additional chemicals. Consequently, the overall pace of standard setting is slowed. Not only must OSHA devote more of its effort to defending its standards from judicial and legislative attacks, thus pulling resources away from additional standard-setting endeavors, but the process of setting new standards becomes more burdensome because evidentiary requirements for new standards become stiffer and stiffer, in anticipation of future litigation and political assaults. Paralysis ensues. A few dangerous chemicals get overregulated and hundreds are not regulated at all.

This is in fact what appears to have happened with OSHA. In its nearly two decades of existence, OSHA has promulgated human exposure limits for only 10 toxic chemicals, while the American Conference of Governmental Industrial Hygienists (ACGIH) has, in the same period, set exposure limits for several hundred. This disparity, according to Mendeloff, reflects the extent of OSHA's underregulation.

Most proposals for regulatory reform, Mendeloff claims, address only the overregulation problem, but the underregulation problem is at least as serious if not more so. And the two are linked. Any proposals for reform ought to deal with both. Mendeloff has designed a three-part reform proposal to handle both horns of the dilemma simultaneously. He argues that OSHA "would be better off setting standards less strictly but more extensively" (p. 2). To enable this, Congress must amend the OSHA Act so that it would (1) allow OSHA to balance costs and benefits in setting standards, (2) allow OSHA more flexibility when setting chemical exposure limits, and (3) require OSHA to adopt all the ACGIH changes in exposure limits since 1968.

Allowing OSHA to balance costs and benefits in its regulatory decisions would enable the agency to make more sensible estimates of where best to deploy its resources to achieve the greatest amount of health protection. Allowing cost-benefit balancing would also open the door to greater flexibility in the standard of proof needed to support agency actions. If more evidentiary flexibility were permitted, then OSHA could act more expeditiously in cases in which the potential hazard is high but control costs are low, while at the same time maintaining a high standard of proof in cases where control costs are high but hazard potential is thought to be low. The third prong of Mendeloff's proposal, adopting ACGIH's standards, would bring OSHA up to speed immediately for the host of still-unregulated chemicals in the work place. This provision would overcome one of the principal political barriers to adoption of the rest of the proposal, which is the legitimate worry on the part of those who are concerned about worker protection that the new flexibility would only result in lowering existing strict standards without bringing about any expansion of coverage.

Mendeloff's explanation of how overregulation causes underregulation of toxic chemicals is well crafted and persuasive. It rests primarily on an intricate but clearly presented cost-benefit analysis that is sensitive to both the strengths and weaknesses of economic approaches to public policy. For those who teach public policy analysis, Mendeloff provides a superb illustration of how the tools of economic analysis can help us distinguish things that cry out for regulation from those that are merely regulatory resource eaters.

Mendeloff's argument regarding appropriate remedies is somewhat less well developed and less convincing than his explanation of the roots of the underregulation problem. Although his three-pronged proposal for reform appears eminently reasonable, it is difficult to share his optimism regarding chances for passage through Congress. His analysis of political feasibility is less incisive than his analysis of economic efficiency. Following the revered principle that a bird in the hand is worth two in the bush, the probable chief opponents of change—unions—will rightly raise questions about unanticipated consequences. (After all, isn't our current underregulation an unanticipated consequence of overregulation?) Good as Mendeloff's arguments are, it will likely take more than these to overcome opponents' reticence.

Nevertheless, Mendeloff has provided the strongest of support for an important insight into contemporary problems of social regulation: overregulation can cause underregulation. Future efforts at reform of regulatory agencies such as OSHA and the Environmental Protection Agency would do well to consider Mendeloff's proposal seriously and to undertake a more extensive analysis of its political feasibility.

KAREN B. WILEY

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The Power of Public Ideas. Edited by Robert B. Reich. Cambridge, MA: Ballinger, 1988. 265p. $19.95.

The Power of Public Ideas displays a rare combination of virtues, building an innovative approach to public policy on a foundation of old-fashioned common sense. It is, as a result, an extremely *useful* book. It identifies what is important and what is trivial, what is true and what is silly, what we know already and what we need to know about the sources of public policy.

The book is the result of what is apparently an ongoing conversation among several members of the Kennedy School faculty at Harvard, joined by a few colleagues from the outside, about the currently dismal state of policy analysis. I realize that for many people this description of the book will not seem promising: Harvard has a reputation for being somewhat insular; and in any case, if there is something wrong with the profession of policy analysis, the Kennedy School—one of the profession's major vocational schools—must bear part of the blame.

But for whatever reason, the contributors to this volume have lost their patience with the major intellectual props of contemporary policy analysis, which are, as they see them, the public choice school of economics and the interest group school of political science. In his introduction, Robert Reich describes the prevailing philosophy of policy making as follows:

> People are essentially self-interested rather than altruistic and behave much the same way whether they are choosing a new washing machine or voting on a new board of education. These personal preferences are not significantly affected by politics, social norms, or previous policy decisions. The public good, or "public interest," is thus best understood as the sum of these individual preferences. Society is improved whenever some people's preferences can be satisfied without making other people worse off. Most of the time, private market exchanges suffice for improving society in this way; public policies are appropriate only when—and to the extent that—they can make such improvements more efficiently than the market can. Thus the central responsibility of public officials, administrators, and policy analysts is to determine whether public intervention is warranted and, if so, to choose the policy that leads to the greatest improvements. (p. 1-2)

As an antidote to the public choice-private interest model, Reich and his colleagues propose a radically simple idea: people are motivated by ideas of right and wrong, and it is therefore useful to understand what these ideas are, where they come from, and whether they are true. Furthermore, citizens are motivated by attachments that have very little to do with their "interests" in the conventional sense. Gary Orren, for example, points out that "efforts to explain U.S. voting patterns in terms of economic self-interest have . . . failed consistently. 'Solidary' factors . . . and 'purposive' goals . . . are far more influential" (p. 26). And Steven Kelman makes a convincing argument that both citizens and public officials are frequently motivated by "public spirit," that is, "the wish to choose good public policy, evaluating options against a standard of general ideas about right and wrong" (p. 31).

That's the commonsense part, and it is a measure of the damage done by the public choice school that such simple truths will strike many readers—and not only undergraduates—as novel. The creative part is the application of this common sense to the process of policy making. Mark Moore describes how unexamined assumptions about the nature of public problems—drunk driving, for example—can suddenly undergo scrutiny, leading to a general reassessment of previously tolerable private behavior. Philip Heymann argues that public policies are not simply, or only, instrumental solutions to carefully described problems: they also express the public's more general notions about what kind of society they want to live in, what things are right and wrong, and what values the government should nurture or protect. Robert Reich urges public managers to "stimulate public debate about what they do" so that the general public can be helped to "discover latent contradictions and commonalities in what it wants to achieve" (p. 124). Reich calls this the process of "civic discovery," arguing that "the failure of conventional techniques of policy making to permit civic discovery may suggest that there are no shared values to be discovered in the first place," a message that "may have a corrosive effect on civic life" (p. 146–47).

This is a necessarily small sampling of a rich collection, and I think that policy specialists as well as students will find it helpful. It clears away a lot of deadwood and turns the discussion of public policy toward the things that really matter and the ideas that are worthy of public and professional deliberation.

DENNIS HALE

Boston College

The Impact of Policy Analysis. By James M. Rogers. Pittsburgh: University of Pittsburgh Press, 1988. 202p. $26.95.

Rogers begins *The Impact of Policy Analysis* by posing a central dilemma of contemporary politics. "The governance of modern industrial society is a complex task fraught with uncertainty heightened by ignorance. . . . Knowledge is frequently lacking and is costly, if not impossible, to obtain. Nevertheless, nations are compelled to act" (p. 3). In this context, he identifies a critical question for any professional policy researcher or policy maker: Does knowledge gained through policy analysis make a difference—in his words, is there a discernible impact—in the formation and conduct of actual policy? Although others, such as Carol Weiss and Nathan Kaplan, have proposed a generally discouraging answer to this question, Rogers promises to look at the question in terms of a rigorous paradigm, testable hypotheses, and case studies materials, thereby providing a much more analytically supportable conclusion. Surely a worthy and most beneficial endeavor, with answers relevant to most of the policy research communities.

Unfortunately and not entirely unexpectedly, he falls distressingly short of his mark in a number of ways. Nothing he writes is patently wrong but much of it is disturbingly incomplete. For instance, his review of the intellectual and disciplinary underpinnings of policy analysis (which, in fact, is unnecessary to his analysis) is compressed into a dozen cursory pages. His principal source of evidence and example—the negative income tax (NIT) experiments—is largely left to the reader's prior knowledge, as Rogers himself admits: "I am making a long and somewhat complex story much too short" (p. 98) and "my brief and incomplete history of the negative income tax" (p. 99). At less than 150 pages, this book scarcely threatens the reader's endurance. Why, then, the brevity, which does threaten the reader's understanding?

To Rogers's credit, he does reflect the complexity of the policy process and the differing objectives of the various participants. Thus, the assessment of the impact of policy analysis is in itself a perplexing calculus. His "variables" include value positions, institutional biases, intensity, and belief systems. But he treats these as somewhat isolated variables, expositionally neglecting their interactions. More damaging, however, is that his propositions are essentially trivial. Who would question his major finding that the firmer the prior beliefs and positions (held by either individuals or institutions), the less impact obtained from analysis?

Rogers's presentation of his argument is equally problematic. He sets out his hypotheses rather neatly, but the reader has little indication how he arrived at these propositions. His explanation and review of the NIT history is scandalously incomplete, a strange oversight since subsequent chapters reveal the missing information in some detail. One must therefore question the author's organization of his material.

A methodological concern is Rogers's case study approach. He presents an exceedingly elaborate nexus of hypotheses (I counted 27—chap. 4) and then proceeds to "test" a number of them in the context of a single case study, the NIT. We are given little clue as to why some are tested while their kin are ignored. Even leaving aside the propriety of such an approach, one should ask—as Rogers studiously does not—if the NIT example is representative. Well-couched arguments against this thesis could easily be imagined, which, if upheld, would relegate Rogers's work to the dustbin of academic rhetoric in the most unfortunate sense of the word. This gnawing suspicion is reinforced when Rogers concludes, "It should be remembered that the [proffered] explanation is untested and may later be wholly or partly falsified" (p. 148).

My greatest problem with *The Impact of Policy Analysis* is that Rogers stops far short of where he should, that is, he offers little except well-intended criticism. He virtually states that analysis on important policy questions is irrelevant, but fails to ask—let alone substantively answer—the next and counterfactual question, What would policy look like without an analytic contribution? What are the costs to the policy process if it were to surrender its policy research capabilities? If Rogers takes his pessimistic findings seriously, he should be impelled to offer remedies. Yet there are none, no way out of the binds Rogers has wrapped around the policy research community.

However worrisome these problems are, the book is not without its merits. The chapter overlaying Rogers's policy impact template with his NIT evidence contains some original research and striking examples of the use and misuse of policy research by the contending stakeholders, for example, how the very same analysis can be used to support contradictory positions or how it might be used in the long (as opposed to the short) term. Still, there is a heavy price for the reader to bear.

PETER DELEON

University of Colorado, Denver

Book Reviews: American Politics

The Party's Just Begun: Shaping Political Parties for America's Future. By Larry J. Sabato. Glenview, IL: Scott, Foresman, 1988. 261p. $24.63.

This excellent brief book contains all the essential facts and figures that interested citizens might want to know about the two major political parties in today's United States. The book begins with a capsule party history. It describes their role in the various branches of government at the federal, state, and local levels. It tells how parties are organized and financed and how they operate. Larry Sabato stresses the importance of parties for U.S. democracy. He explains in simple, lucid prose why they must be vigorous and why the alternatives to party political control are unsatisfactory.

Unlike many recent books that rely on secondary analysis of outdated public opinion data, Sabato commissioned three national random sample surveys in 1986 to collect the opinions of adult U.S. citizens about various aspects of the U.S. party scene. Like other scholars, he found that partisan loyalty has weakened over the years. Although he regards the change as a serious political problem, Sabato remains more optimistic about the status of the United States' political parties than many other scholars. He points out that most U.S. citizens still acknowledge the importance of parties and use them as yardsticks in their political decisions. People may be apathetic and neutral, but they show no enmity or antagonism towards the current two-party system.

To strengthen parties for the future, Sabato suggests reforms that the parties can accomplish either on their own or with the help of government. On their own, parties can and should increase services to voters and to candidates to make them feel that parties are important and fill needs that other institutions cannot satisfy. To make the parties more visible, Sabato recommends institutional advertising by the parties on a year-round basis and sponsorship of presidential debates during presidential election campaigns. To give each party the benefit of experienced political leadership, he recommends including more elected officials in the presidential nominating conventions as unpledged, unbound superdelegates.

Among reforms that require government action, Sabato suggests greater independence of the parties from government controls to allow them to choose their own structures and rules and devise their own nominating methods. Despite the popularity of primaries, which weaken party control, Sabato believes that recent court decisions reflect a political climate that favors greater party autonomy. To assist parties financially, tax credits might be provided for small donors, and expenditure ceilings might be eased. Financial contributions might be channeled through the parties, which should be allowed to keep a portion for their own expenses. Additional patronage positions might be made available as rewards for the faithful. To make parties more visible to voters, ballots could be redesigned to stress party labels and ease party voting, and party registration could be mandated in all states. Nonpartisan elections and third parties should be discouraged. Allotments of free television and radio time for national and state parties should be expanded to a minimum of one hour per year, usable in small time segments in prime time slots.

Sabato presents a carefully reasoned, amply documented case on behalf of U.S. parties. His reform proposals are well within the realm of feasibility. As he points out, all that is needed to bring them to fruition is the will to do so. But this is precisely the problem. Available evidence indicates that party reform is of little concern for average U.S. citizens. Sabato's book will make his readers more knowledgeable about the parties. His pleas for reform will gladden the hearts of champions of the U.S. two-party system. But his dispassionate message is not likely to arouse the fervor among the uncommitted that he deems essential to assure a vibrant future for the donkey and the elephant.

DORIS A. GRABER

University of Illinois, Chicago

The Economy of the Earth. By Mark Sagoff. Cambridge: Cambridge University Press, 1988. 271p. $29.95.

Mark Sagoff's book is not about the earth's economy or international economics as his title suggests. There is nothing global about it, unless it is his eclectic approach to discussing current environmental issues in the United States. In coming to grips with such intractable technical and political problems as water and air pollution, preserving wilderness areas, and cleaning up toxic dump sites, Sagoff ranges widely into such bodies of scholarship as political theory, public law, history, and economics. It is a praiseworthy effort on Sagoff's part, but unfortunately the book also demonstrates that interdisciplinary environmental studies are still in their formative stages.

Sagoff's book *is* about national environmental problem solving and the various approaches advocated and used by policy analysts to resolve these issues. Here is his perspective, taken from chapter 1:

> This book argues for an ethical, aesthetic, and cultural interpretation of the goals and purposes that underlie social legislation, and it explains how such an interpretation can help us adjust those goals to the economic, technical, and other constraints that might otherwise prevent us from achieving them. It contends that the conceptual vocabulary of resource and welfare economics . . . which once served to justify social, especially environmental, policy, has largely outlived its usefulness and has become a distraction and an important obstacle to progress. This book presents a philosophical defense of one conception of social policy, then, and a philosophical critique (I should say, an autopsy) of another. (p. 1)

Each of the eight succeeding chapters critiques various aspects of the public choice school of economics with the same precision and clarity evidenced in the paragraph quoted above. Chapter 3, for example, shows how "consumers" and "citizens" may have different preferences and thus make different decisions regarding the same question. The now-moribund controversy over developing California's Mineral King Valley into a Disney resort is used to illustrate how as consumers we may want ski lifts and souvenir shops while as citizens we may vote for solitude and roadless areas. If the decision whether to develop the Mineral King area had been left to public choice economists, Sagoff argues, we would now have a high-country Disneyland, since market analyses indicated the need for more ski areas in the Sierra Nevadas.

Other chapters lay out the shortcomings of cost-benefit analysis, the Lockean conception of property rights, shadow pricing of intangible values, and the limits of utilitarianism. There is a lot of repetition in the first five chapters. Always the preferred approach is what I will call (since Sagoff never names it, as far as I can tell) Arendtian, after the great twentieth-century political theorist, Hannah Arendt. Her conception of the state and of citizenship was informed by classical Greek philosophy, where citizenship and community *meant something.* In effect, her political thought makes the careful reader yearn for a genuine political community, though these yearnings invariably shatter against the hard-rock reality of late-twentieth-century U.S. politics—the uncivic culture. However, Sagoff claims in this book that contemporary environmental issues revive these ancient modes of thought and behavior, and that the latter will replace the arid conceptions that rational choice theorists, libertarians, and Pinchot-type *utilitarians* have espoused for long enough. Perhaps.

Though this thread of criticism runs throughout the book, it cannot be said that a coherent, well-developed argument is presented by Sagoff in *The Economy of the Earth*. Rather, the work reads like a series of essays loosely related by a substantive concern with environmental policy and a dislike for public choice theory. Chapter 6, "Nature and the National Idea," seems barely related at all. Set in the middle of the book, it is a familiar overview of the work of such famous U.S. historians as Perry Miller, Roderick Nash, and Leo Marx. It contributes very little to the book's content, particularly when such mystifying phrases as the *environing wilderness* (p. 139) are used.

In two later chapters, entitled "Can Environmentalists Be Liberals?" and "Where Ickes Went Right; or, Reason and Rationality in Environmental Law," the author not only misinterprets the philosophy of a central figure in the Progressive conservation movement, Gifford Pinchot, but puts him at the head of the U.S. Forest Service in the 1930s, when Harold Ickes was Interior Secretary and was trying mightily to get control of that powerful agency. Pinchot, who was the first chief of the Forest Service under President *Theodore* Roosevelt, did in fact fight Ickes tooth and nail throughout the New Deal years to keep forestry in the Agriculture Department, but that was decades after Taft dismissed him as chief. (He won that battle, incidentally). Had Sagoff read

Pinchot's autobiography, *Breaking New Ground,* he would not have made this error, but more importantly, he would have gained an appreciation for Pinchot's philosophy of conservation, which was not very different, after all, from that of his friends and fellow executives, Harold Ickes and Franklin Roosevelt.

Sagoff's book will be appreciated most by scholars of a normative inclination who are uncomfortable with a lot of contemporary policy analysis. *The Economy of the Earth* is, as Sagoff states, an alternative. He calls it an "ethical approach" (p. 4) to social and environmental policy determination, though I suspect that certain scholars of ethics would disagree with that characterization.

JEANNE NIENABER CLARKE

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Balanced Budgets and American Politics. By James D. Savage. Ithaca, NY: Cornell University Press, 1988. 350p. $27.95.

Questions of budget deficits, constitutional amendments for a balanced budget, Reagan's request for item veto authority, and reliance on huge continuing resolutions have dominated the 1980s. There is little relief in sight. This book provides an excellent context and historical backdrop for these battles.

James Savage argues that the U.S. concern for balanced budgets is rooted in politics and symbols rather than economic doctrine. By balancing the federal budget, Jeffersonians and Jacksonians hoped to limit the size of the national government and promote republican virtue. Savage regards the effect of budget deficits on inflation, interest rates, and trade deficits as a contemporary fascination.

Savage does a good job in describing the inconsistencies and contradictions within the Republican and Democratic parties over balanced budgets. Neither party promotes a reliable, coherent theory about the relationship between the budget and the economy. Ronald Reagan came into the White House by promising a balanced budget by fiscal 1984. Instead, he presided over the largest growth of budget deficits at any time. While calling for a constitutional amendment to balance the budget, he nonchalantly sent budgets to Congress that projected annual deficits of $200 billion for years to come. When he entered office, the total national debt was about one trillion dollars. As he left, it was approaching three trillion. The deep, almost-religious commitment by Republicans to balanced budgets was tossed to the wind.

For their part, the Democrats tried to jettison their image as "big spenders" and behave like prudent fiscal managers. Walter Mondale's pledge in 1984 to raise taxes to balance the budget was answered by a landslide defeat. Savage criticizes Mondale's promise to cut the deficit by two-thirds by the end of his first term: "But why reduce the deficit by two-thirds? Why not three-quarters or one-half, or eliminate it altogether?" (p. 229). The Democrats, rather than distinguishing between deficits to be tolerated during a recession ("passive deficits") and deficits to be restrained during better times, "simply no longer employed an agreed-upon measure to distinguish good from bad deficits, one that responded to changes in the economy and guided fiscal policy. Instead, they came to rely on fixed standards that lacked any intrinsic economic justification and were altered as the situation demanded" (p. 229).

These critiques of the two parties are well aimed. The author is also on target by stating that the early Reagan deficits probably "lessened the recession's harsh effects and assisted the economic recovery of 1983–1984" (p. 233). But the book implies that the campaign against budget deficits is basically political and symbolic, with little economic foundations. Why not deficits of $400 billion or more? Is there no point when deficits are indeed harmful? Are we enjoying short-term benefits at the cost of long-term penalties?

The book would have been more instructive if it analyzed the risks of budget deficits. In 1974 Nixon's budget showed 7% going for interest on the public debt. By the time of Reagan's budget in 1988 the figure was up to 14%. With restraints on domestic and defense spending and the continued growth of the national debt, an even higher percentage of the federal budget will be set aside simply to pay interest on the public debt. Increasingly, there is less flexibility in choosing spending priorities.

The author does not identify the economic events that allowed Reagan to run up massive deficits without triggering inflation and high interest rates. Other than a brief reference to oil prices on page 192, Savage fails to mention that both Ford and Carter were clobbered by OPEC policies on oil. To the great luck of Reagan, whether because he's Irish or Aquarius, the cartel fell apart and oil prices tumbled. Also to Reagan's advantage were worldwide decreases in agricultural and food prices. Labor unions had less leverage to bargain for wage increases. These are secular trends and cannot be attributed to conscious policy by the Reagan administration. Reagan caught a good wave and rode with it.

Moreover, as Savage acknowledges, much of the fight against inflation can be attributed to the Federal Reserve and Paul Volcker, not Ronald Reagan. Part of the cost of this policy has been high interest rates, high enough to attract foreign investors since we depend on them to help finance the federal deficit. As a result, we are more vulnerable to the decisions of foreign investors; more of our capital stock belongs to foreigners; and the returns on those investments will go to foreigners, not to us. There is serious concern that heavy borrowing will limit modernization, economic expansion, and improvements in U.S. living standards. These spinoffs and repercussions from Reagan's budget deficits merit close and careful study.

LOUIS FISHER

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Behind Baake: Affirmative Action and the Supreme Court. By Bernard Schwartz. New York: New York University Press, 1988. 266p. $35.00.

In an age of "behind-the-scenes" books, one cannot be surprised to see books purporting to tell the reader what goes on behind the red velour draperies in the Supreme Court. Thus, in recent years we have been enlightened by such books as Woodward and Armstrong's *The Brethren* (1979), Schwartz's earlier *Super Chief* (1983), and OBrien's *Storm Center* (1986). Though not intended to be a mini-*Brethren*, *Behind Baake* is presented as a behind-the-scenes analysis of *Regents of the University of California at Davis* v. *Baake*, a case involving Alan Baake's claim that a minority-preferenced quota system used in determining admissions to the medical school at Davis was unconstitutional. Essentially, *Behind Baake* traces the development of the case from the California courts through final decision in the Supreme Court. Most of the book is devoted to the higher Court and considerable reliance is placed on interviews with justices, clerks, and others, along with memoranda and other documentary material furnished to the author—some of which is included in an appendix.

The stated purpose is "to give students of the Supreme Court further insight into the Court's largely unrevealed decision process" (p. ix). In pursuit of that purpose, the author gives glimpses of matters or events that are not widely known. For example, we are told that Brennan, Marshall, Blackmun, and Burger were opposed to granting certiorari in the case. (Brennan believed that the presence of a "quota" would encourage the majority to rule that race could never be a factor in admission decisions.) We are also told that Marshall wanted Brennan "to find some pretext to get rid of the case [after certiorari was granted] because he believed that quotas were often indispensable to affirmative action programs" (p. 42), that after certiorari was granted Brennan wrote some of his 1976 term opinions with *Baake* in mind, and that the government's brief was rewritten in the White House without knowledge of Solicitor General Wade McCree. While these are interesting tidbits, the most significant new insights offered in the book pertain to the role of Justice Brennan. The author's interpretation of the events in the Court as the case unfolded "behind the scenes," and his assessment of ultimate contributions to decision in the case depict a key role for that justice. While Justice Powell wrote an opinion he considered the most important of his career, an opinion that has attracted wide media and scholarly attention, the hero of *Behind Baake* is clearly Bill Brennan.

At a conference on the merits on 9 December 1978, the author reports a voting split of Burger, Stewart, Stevens, Rehnquist, and Powell to affirm the California court decision requiring Baake's admission to the medical school with Brennan, Marshall, and White in dissent, and Blackmun absent. At conference Powell indicated he would vote to affirm the ruling of the California Supreme Court, a ruling restricted to the Equal Protection clause. At this point

Justice Brennan is credited with interjecting remarks that convinced Powell to affirm in part and reverse in part—an approach first recommended by a Brennan law clerk. Brennan's purpose—to ensure that race as a factor in admissions would not be ruled out totally in the future—was realized when Powell agreed, on the spot as it were, that "the judgment must be reversed insofar as it enjoins Davis from taking race into account" (p. 96). Thus, Brennan is said to have prevented the "anti-Davis justices [from securing] a complete victory in the case" (p. 147).

In addition, Brennan is said to have obtained an even more significant victory. For given the multiple opinions and positions represented by the justices, the "central meaning" of the case might have been obscured. Brennan sought not only to clarify that central meaning but to give it a "spin" that would effectively salvage affirmative action programs.

For Brennan, the central meaning of the Court's decision was that government "may take race into account when it acts not to demean or insult any racial group, but to remedy disadvantages cast on minorities by past racial prejudice, at least when appropriate findings have been made by judicial, legislative or administrative bodies with competence to act in this area" (pp. 139–40). Since Powell did not agree with this position, Brennan's view was not that of a majority. Nevertheless, the author suggests, Brennan's continual assertion of this view in communications with his colleagues, in his written opinion, and in his public summation on decision day, coupled with Powell's public silence on the question, has permitted Brennan's view to "all but take over the field" (p. 147).

As a consequence, Schwartz believes, colleges and universities nationwide have been able to continue to provide preferential treatment to minorities in their admission policies with little if any impediment from the Court's rulings in *Baake*.

Assessing the author's stated goal of providing "further insight into the Court's largely unrevealed decision process" (p. ix), my conclusions are mixed. If one can lay aside the author's apparently heavy reliance on Justice Brennan for many of the key facts provided in his behind-the-scenes scenario, one will gain further insight into the particular decision process that produced *Baake*. But if we ask whether our understanding of the Court's decision processes in general has been enhanced, it is harder to say *yes*.

The kind of influences (personal, policy, etc.) at play behind the scenes in *Baake* will surprise no close student of that institution. From Mason's *Harlan Fiske Stone: Pillar of the Law* (1956) to the present, any number of books and articles have detailed how the kind of extralegal considerations discussed in *Behind Baake* have shaped important Supreme Court decisions. If doubt lingers, a quick perusal through the papers of any number of justices will quickly dissipate it. Thus, the book will be of greatest value to those who wish to know more about *Baake* as a unique case and of least value to knowledgeable students of the Court who seek a broader understanding of Court processes.

S. SIDNEY ULMER

University of Kentucky

The Power Game: How Washington Really Works. By Hedrick Smith. New York: Random House, 1988. 793p. $22.50.

So many books of the "inside Washington" genre have appeared in recent years that one could hardly have hoped for a fresh and revealing treatment. Yet Hedrick Smith, longtime *New York Times* correspondent in Moscow and later in Washington, offers in *The Power Game* a cornucopia of new insights, fascinating anecdotes, hard information, and important conclusions. For years to come political scientists will be mining this volume for their teachings and writings in U.S. government.

Smith uses *power game* not simply as a metaphor but as a way of understanding "how government actually works and why it does not work better" (p. xiv). By "game" he does not mean that politics is child's play: "Washington is a world where substance matters. Issues matter. Ideas matter. . . . But Washington is a city engaged simultaneously in substance and in stratagems. Principles become intertwined with power plays. For Washington is as much moved by who's up and who's down, who's in and who's out, as it is by setting policy" (pp. xv–xvi). The author, in a brilliant treatment of "the big games of power," applies this concept to agenda making, image shaping, coalition building, opposition sustaining, and foreign policy making both through the bureaucracy and through "end runs."

His searching examination of the back channels as well as the corridors of power illuminate phenomena little noted in the traditional literature. He discusses "floating power" that appears and disappears, fashioning quick connections and disconnections, among the power centers, within a rigid constitutional structure. He describes "porcupine power" – the influence accruing from simply being difficult and negative, a curmudgeon. He analyzes "narrow casting" (targeted mail to subgroups) as opposed to broadcasting to the general public; the tendency of "new collar" voters to split their tickets; the use of secrecy in government that tends, even aside from its other evils, to shut off policy disagreements that should be aired.

While Smith deals with the Carter and immediately preceding administrations, he treats in particular detail the gamesmanship of the Reagan White House. He gives Reagan full credit for leading the Republican party back into presidential power and for striking successes during his first year in office. But his portrait of Reagan—based on numerous interviews as well as documents such as the Tower Commission report—is of his erratic supervision, faltering policy leadership, outright deceptions, and evasion. On foreign affairs Reagan "would set a general course, but his understanding was often so hazy and simplistic that he was at the mercy of aides – more than most presidents. Republican senators, coming away from the Oval Office, told me of their shock at his weak grasp of important issues. Most Reagan aides loyally defended him, but some admitted embarrassment at his gaps or his mental laziness. Others reported having to step in and handle conversations for Reagan so that he did not look stupid" (pp. 627-28). No wonder Iran-gate.

Smith sees major failures of the system as stemming from the "earthquake" in presidential-congressional relationships following Vietnam and Watergate. Some scholars of U.S. government will view the basic disarray of the system as having emerged full-blown much earlier in this century. They may note a kind of "gee-whiz" reaction on Smith's part to problems that more jaded political scientists have long recognized and even accepted. On the other hand, Smith makes full use of the work of many political scientists, most notably E. E. Schattschneider and James L. Sundquist; and even the more cynical reader will share the author's sense of wonder and discovery as he unearths the latest cover-up or other shenanigans in Washington.

The book has been criticized for being long on diagnosis and short on prescription, but in fact Smith devotes over fifty pages to an arresting set of conclusions and proposals. Like a number of journalists before him, most notably David Broder, he hopes for the restoration – or creation – of a strong party system. "One basic problem," he writes, "is that we have no majority party. The old New Deal Democratic coalition has been broken up without being replaced by a solid Republican majority. Effective political parties are vital to governing, to giving shape and coherence to policy making. Yet the public disdains and discounts parties, the Constitution does not mention them, and the founding fathers were wary of what James Madison called the 'dangerous vice' of partisanship and 'sinister combinations' " (pp. 652–53). He pictures a two-party realignment in which the Republicans under Reagan and Company became the solidly conservative party of the nation while the Democrats remain half realigned, half paralyzed.

Given more dealignment than realignment, colossal nonvoting, the power of the media and of political action committess – all of which Smith ably describes – what can move the system off dead center toward the kind of cohesive, programmatic, responsible government that Smith and many of his readers might favor? Would constitutional change as well as party renewal be necessary? Smith rather sympathetically lists proposals by the Committee on the Constitutional System and others: the four-year term for representative and eight-year term for senator synchronized with presidential terms; the "team ticket" that would put president, vice president, senator, and House member on the same slate, requiring, or at least enabling, a straight party ticket vote; "bringing back the pros," such as the Democratic party "superdelegates" who would lend more balance, coalition-building skill and professionalism to governing; strengthening party finance and using the parties as major conduits for television subsidies to candidates, thus mitigating the individualist, "king of the hill" behavior of office seekers campaigning separately from their parties.

All this is a prescription for a stronger, more institutionalized majority and indeed for a stouter party opposition. Yet toward the end Smith draws back from this course. Like the framers of old, he worries about excessive partisanship. He lauds the basic soundness of a recent proposal that governmental deadlock be overcome through a bipartisan ticket in presidential campaigns: president from one party, vice-president from the opposition party, cabinet drawn from both. After a long and brilliant description of the fundamental irresponsibility of the U.S. political system, with its finger-pointing and buck-passing elections and its confused sharing of power, the author would apparently institutionalize a system that erodes authority and responsibility even further. If this desire for majority rule and ultimate fear of it comprise a fundamental ambivalence shadowing the author's conclusions, however, it is one that he shares with much of the public and indeed with many political scientists.

James MacGregor Burns

Williams College

Money in American Elections. Frank J. Sorauf. Glenview, IL: Scott, Foresman, 1988. 416p. $12.95 paper.

For those interested in political cycles, there is one of sorts in the study of campaign finance: some 28 years after the "discovery" of campaign finance in 1904, the first full-scale scholarly treatment of the subject appeared, Louise Overacker's *Money in Elections* (1932). It was not superceded for another 28 years, until the publication of Alexander Heard's *Cost of Democracy* (1960). And now, 28 years later, Frank Sorauf's *Money in American Elections* is a worthy successor.

Over the last 84 years the study of campaign finance has emerged as a separate field in U.S. politics, and Sorauf describes and defines the field as it is presently understood. Students of campaign finance at all levels can learn a great deal from this book. It is systematic and very well written, sensitive to the nuances of the subject matter, in sharp contrast to standard muckraking treatments, such as Philip Stern's *The Best Congress Money Can Buy* (1988), which obscure as much as they reveal.

Not that Sorauf ignores the persistent charges of corruption and unfairness nor the continuing search for reform. He rigorously applies logic and evidence to these questions, covering in a detailed, yet focused, manner subjects and issues often left unexplored. Among the discrete topics covered are individual contributors, political action committees (PACs), parties, and campaign spending at the state, congressional, and presidential levels. As a bonus, he deals with the politics of campaign finance regulation, including the role of the news media, the Courts and the Federal Elections Commission, as well as the Congress and public interest groups. His discussion of each topic bristles with insights and testable hypotheses.

The real gem, however, is his outline of the campaign finance "system" itself, in which he traces political money from its origins to its eventual impact on U.S. politics. Indeed, one wishes this treatment appeared earlier in the book, to provide more cohesion to the topical material. Sorauf provides a concise exposition of the principal conclusions of recent scholarship: "checkbook pluralism," practiced among thousands of PACs and millions of donors, usually reduces the influence of any one source of funds from a bang to a whimper. Hence money does not so much "talk" as "whisper" in politics, echoing other, more important, sources of political power.

In addition to summarizing and synthesizing what is known about campaign finance, Sorauf suggests areas for future research, including the following.

Data is destiny. Unlike previous eras, Sorauf points out, today's scholars are confronted with a vast amount of campaign finance data, much of which is ill defined, poorly organized, and difficult to use. Nonmonetary variables are even more poorly conceptualized and measured. And there are still serious lacunas, particularly at the state and local level. Thus, a great deal of basic research remains to be done. Whether one believes that money "talks" or "whispers," increasing the quality of campaign finance data is essential.

The money of power. Scholars can usefully move beyond considering political money in and of itself and use campaign finance data to study the distribution of power in U.S. politics. Sorauf raises some intriguing questions along these lines. For instance, what does the rise of PACs tell us about the influence of the middle-class citizens, who are the largest source of political money? What are the characteristics of the national, rather than local, "resource constituencies" tapped by modern fund-raising organizations? Whether one accepts "checkbook pluralism" or not, money is one way to track political power.

From attacking myths to addressing them. Although the mythology of political money is often impervious to evidence, scholars share with muckrakers and reformers many criticisms of the present system. Sorauf asks some pertinent questions on this score as well. For example, what influence have increased campaign expenditures had on the quality of campaigns? What effects do fund-raising methods have on kinds of candidates recruited to run for office? If political money is to be regarded as more than a necessary evil, scholars must show its connection to the central values of representative democracy and thus to the common sense of the public.

These topics, among others, should keep scholars occupied for some time—at least 28 years—when, if history is any guide, the next seminal book on campaign finance is due to appear.

JOHN C. GREEN

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Political Mythology and Popular Fiction. Edited by Ernest J. Yanarella and Lee Sigelman. Westport, CT: Greenwood, 1988. 200p. $37.95.

The metamorphosis of U.S. politics into popular fiction makes timely the publication of this book in the last year of the Reagan administration. *Political Mythology and Popular Fiction* is concerned, however, with political ideas in novels and short stories rather than with the breakdown of the boundary between politics and fiction in political debate or with the hold of political fantasies on masses and political elites. Attention to political myths unifies this collection, according to the editors, who see their contributors as establishing, demystifying, and reconstructing the dominant ideas of U.S. public life. But instead of a set of exposés of the political myths that dominate popular fiction, these essays share almost the opposite project, for they tend to redeem popular fiction as critical of U.S. politics. Most counterpose admirable fictional communal aspirations to the mobile individualism celebrated in political life. This volume seeks to expand to fiction the range of legitimate political investigation, and it mostly does so by setting communal literature against Lockean politics.

Appropriately enough, the collection begins in the West. John Moeller sees the western as clearing space for social action, not as glorifying the individual hero alone in nature. Ethan Fishman uses the tales of John Cheever and John Updike to warn against the absence of moral obligation in the middle-class United States. Thomas Shevory finds the sports novel either holding out communal relations in a predatory U.S. environment, or else as showing how sports have fallen victim to predation. Samuel Hines contrasts James Michener's apotheosis of U.S. communities over time to Gore Vidal's corrosive vision of U.S. history. Timothy Cook, in a particularly interesting contribution, contrasts the initiative and community self-creation displayed in U.S. children's books to the acceptance of hierarchy and natural order in their British counterparts. Jean Bethke Elshtain, faithful to the domestic roots of her feminism, criticizes male writers' assaults on small town suffocation and sympathizes with women writers' evocations of small town community. For Ernest Yanarella the pastoral tradition in science fiction stands against the triumph of capitalist technology. And Cecil Eubanks, more critical of what happens to men together than most of his fellow contributors, analyzes the fictional communities of war.

Although these essays respond to the dismissal of myth and literature in political science as tools for understanding political life, they themselves fall victim to the political science division between facts and values, empirical investigation and normative theory, politics and fiction. Great works of art (which may or may not be popular) teach us, through the very particularity of their representations, truths we had not grasped before. Their insights are importantly historical, and analysis can help uncover the specific conditions that illuminate, and are illuminated by, the fiction. These essays, however, are interested less in historicizing and particularizing literature than in allegorizing it. For the most part they plunder art for moral lessons and turn conflicts among fictional characters into struggles between dramatized abstractions. This reduction of art to morality in part accepts the terms of the political science division of labor between politics and fiction. But *Political Mythology and Popular Fiction* also imports political science into the space its literary analysis had supposedly reserved for itself. For one thing, best-sellers drive out lasting works of fiction. Of the great U.S. writers, only Melville, Faulkner, Edith Wharton, and Steven Crane make even fleeting appearances, the first three through one lesser work each. For another, the authors employ a methodology that rushes through numerous works, rarely pausing to analyze any single one in depth. This quantitative approach does establish interesting patterns and is perhaps the one best suited to the kind of literature treated here. It does not simply sacrifice literary to political analysis, however, but also makes fiction less powerful as political critique.

Lockean individualism may be one face of U.S. politics, but moral sermonizing is the other. As these essays sometimes recognize and as the Reagan years should remind us anew, laments for lost community flourish alongside promotional self-seeking. Interest politics and utopian longings dance together round the graves of transformative art and grand political theory. In trying to make popular fiction do the work of political philosophy, this volume offers fresh insights into U.S. public values. But it also places on popular literature a weight of moral and intellectual gravity that the fiction mostly cannot bear.

MICHAEL ROGIN

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COMPARATIVE POLITICS

Bilateralism, Multilateralism, and Canada in U.S. Trade Policy. Edited by William Diebold, Jr. Cambridge, MA: Ballinger, 1988. 206p. $16.95.

Contemporary Quebec and the United States. By Alfred Hero, Jr. and Louis Balthazar. Boston: University Press of America, 1988. 532p. $21.50.

Social Scientists and Politics in Canada: Between Clerisy and Vanguard. By Steven Brooks and Alain G. Gagnon. Montreal: McGill-Queen's University Press, 1988. 151p. $22.95.

Canada is the single most important trading partner and ally of the United States. We trade more with Canada than with Japan and the German Federal Republic combined. Our second greatest foreign trading partner is the Canadian province of Ontario. Canada is our NATO partner and an ally in arms since the early 1940s. It is literally the middle power sitting between us and the Soviet Union. Still, U.S. citizens remain in a state of benign neglect regarding affairs of Canadian or Canadian-American relations. These three new works will help mitigate that ignorance.

Canada lacks the drama, the dash and color that draws and holds attention. A "Canadian cold front" is about the most common expression used in the U.S. media when they are conscious of Canada. The old Rose Marie image of Mounties, dogs, snow, and hockey dies hard. These works fit the Canadian mold. There is little dash, and the colorlessness of some of the writing makes for heavy slogging. Yet, like the gold nuggets of the Yukon, there are very worthwhile things to be found in these works.

They address three different topics, in three different styles, for three separate audiences. *Bilateralism, Multiculturalism, and Canada in U.S. Trade Policy* is a collection of six essays by five experts focusing on the proposed U.S.-Canada Free Trade Agreement and what it portends, even beyond trade. Interestingly it offers a valuable Mexican view, by Gerardo M. Bueno, of North American trade interlinkages. The writing is academic but pithy. Good editing has prevented verbosity. And the appendices are valuable, including the "territorial application-frontier traffic-customs unions and free trade areas" article of the General Argument on Trade and Tariffs. Topics addressed by the authors include why Canada approached the United States with the free trade initiative, how the initiative fits into the current Uruguay Round of multilateral trade negotiations, and the effects of the proposed free trade agreement on U.S. and Canadian bilateral trade relations with third countries, with detailed attention to Mexico. The authors conclude that multilateral trade relations are necessary, and that the United States and Canada must lead the way. The final essay, by the editor, offers significant recommendations for future U.S. trade policy and for the Uruguay Round.

Social Scientists and Politics in Canada: Between Clerisy and Vanguard is not as highly focused and policy-directed. In this work the focus is on Canada and Canadian society alone. Clerisy refers to the corpus of learned men as a whole, to the literati. Social scientists in Canada are examined with special attention to their work and its effects since 1945. Brooks and Gagnon investigated the backgrounds, educations, politics, political involvement, class positions, and other indexes of measurement of Canadian social scientists. There is more than a whiff of Julien Benda's *La trahison des clercs* about this book (1969). Benda is liberally quoted in the introduction, and the schema of his thinking is evident elsewhere.

One part that readers will find particularly interesting deals with the development of the social sciences in the classical educational tradition of Quebec and of their growing relation to Quebec nationalism—and separatism. The way in which anglophone and francophone social scientists differed from each other in Quebec offers insight into the politics of language, academe, and the state unique in North America.

As in the other works, the role of the academic as expert—the role of the social scientist as quasi-official policy advisor or even policy framer—distinguishes the social scientist in Canada from his or her contemporary or colleague in the much larger U.S. society. Here we are academics, almost always observers looking in from the outside. In Canada there is a much greater propensity to utilize the social scientist, to bring him or her inside the system, to establish the clerisy within or at least beside the state structure. And in contemporary Quebec this is true with a vigor unmatched anywhere in the behemoth to the south.

The attention to Quebec in this book is focused on social scientists. The broader view of the *fait francaise* in North America today is offered by Hero and Balthazar in *Contemporary Quebec and the United States.* For the first time in several decades this scholarly work offers an in-depth examination, analysis and evaluation of the most crucial economic, political, and social relations Quebec has outside of Canada, namely, with the United States. The work is based upon a decade or more of rigorous and systematic empirical research focused upon the Quiet Revolution of the 1960s through the Trudeau era and the rise of the *Parti Quebecois* in the 1970s to the return of the Quebec Liberals to power in this decade.

The thesis underlying the book—one that finds itself worked out in the conclusion—is that the enduring nationalist (separatist) sentiment among Quebec's francophones will lead inevitably to the resurgence of the movement for independence for Quebec unless English-speaking Canada—the other nine provinces of the confederation—are willing to accept wide "autonomy" for Quebec within Canada. The new constitutional accord in Canada, granting 'distinct society' status to Quebec, may just fulfill this need. The discussion and analysis of the Meech Lake agreement of 30 April 1987 is one of the best I have seen in recent works on Canadian politics. This is because it concentrates on the Quebec view of the accord and flows into an analysis of likely prognoses for Quebec nationalism, which in turn runs into an examination of "devolution" for Quebec and the impact of that process on U.S. interests. The authors have thought it through rather than just listing the nuts and bolts and dropping the subject.

Students of Quebec and Quebec-U.S. relations will be gratified at the 56 pages of empirical data charted at the end of the book in the appendixes. This compilation of data is valuable in its own right. Part 5 is the shortest part of the book, yet it is the last and the most important. It bears the title "Where to Henceforth?" and is made up of just one chapter titled "Future Trends in Quebec and Implications for the United States." This chapter should be read by all U.S. citizens who will in the future be dealing with or studying Canada in general or Quebec in particular. Those in our clerisy could do much worse than to spend a weekend with this big, meaty, but filling book.

To study Canada is more than to study a people "just like us" or a political system that is a pale reflection of our own. Any or all of these three works will help make this truth evident to student and scholar alike.

GERARD F. RUTAN

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Political Violence and Terror: Motifs and Motivations. Edited by Peter H. Merkl. Berkeley: University of California Press, 1986. 380p. $37.50.

Current Perspectives on International Terrorism. Edited by Robert O. Slater and Michael Stohl. New York: St. Martin's, 1988. 270p. $39.95.

A new subfield in political science rapidly is emerging which focuses on the combination of the two elements captured in the title of Peter Merkl's book, *Political Violence and Terror. International,* in the title of the second book under review, brings in a third element. It isn't necessary to the conceptualization, but it does add sufficiently to the already sensational nature of the subject so as to ensure popular, and now increasing scholarly, attention. Evidence of the explosion of scholarly literature is contained in the citations in these two edited volumes and in the number of papers and panels at scholarly conferences.

Constructing a new terminology also is part of the professionalization process. *Low-intensity conflict* and *low-level combat* or *warfare, targeting, Euroterrorism, nonstate,* and *moral equivalence* are part of the expanding specialized vocabulary. High levels of popular and scholarly interest coupled to professionalization naturally result in course offerings.

Consequently, the state of the art at this point necessitates a summary of where we are and suggested guidelines for significant future research. These two edited volumes, combining the contributions of nineteen scholars, directly and indirectly approach that task with a notable degree of success. Merkl's book is divided into two parts, the first dealing with widely used methods such as ideological, ethnic-cultural confrontation, and events-data. Italy, Northern Ireland, and the Basque area of Spain provides the locales. A more extensive second part uses a psychological or social-psychological approach that focuses on individual motivations and factors. Five cases from Europe, and one each from Latin America and the Middle East provide the geographical settings.

Slater and Stohl are more policy-oriented in their intent and framework stemming from the relation of their scholarly project to the Defence Intelligence College (p. ix). Its "seven critical perspectives on international terrorism" (pp. 5–10) as implemented in the book, however, are not markedly different from the Merkl volume. Martha Crenshaw's chapter on ideological and psychological factors and Ted Gurr's on "Empirical Research on Political Terrorism" are obvious examples. Differences lie mainly in the case study nature of the Merkl volume as compared to the cross-sectional approach of Slater and Stohl, and the attempt by the scholars in the second volume to conclude with policy options or implications.

Various attempts at definition illustrate the length of the scholarly journey already undertaken and the remainder to be covered. Merkl analytically but incompletely attempts to develop an operational definition (p. 19). He quickly disposes of one debatable element by excluding governmental violence so long as it is accorded a legitimate monopoly of violence. Emphasizing that there is a large variety of types, situations and activities, he develops and applies a table of 7 variables to 11 cases (pp. 32–33). This provides a useful framework and documents the diversity of the subject, but does not resolve the definition problem. Authors of the various case studies essentially bypass definitions.

Almost all contributor's to the Slater and Stohl volume provide definitions tailored to their particular concerns. Martha Crenshaw clearly deals with antigovernment groups and terrorism. They are autonomous and clandestine, relying on "symbolic violence that exceeds the limits of what society considers legitimate in order to shock and intimidate rather than to destroy" (p. 43). Ted Gurr adds that terrorism is a tactic of political conflict, a doctrine for inducing political change, and a strategy of political action (p. 116). Michael Stohl provides the widest scope by including state terrorism. His emphasis is on violent political actions that "are intended to influence the victim and or a target group wider than the immediate victim or victims" (p. 156).

Merkl's volume is very successful in its major purpose of relating ideology to political violence particularly in the three chapters on Italy and the two on West Germany, partly because they deal with both left- and right-wing terrorism. The Italian cases agree on the development of Red Brigade terrorism as left-wing disenchantment with Communist party reformism and right-wing terrorism as a rejection of a neofascist parliamentary strategy. Marxist-Leninist, Trotskyite, and Maoist constructs on the Left, and neofascist ones such as by Julius Evola on the Right provide the ideological bases. "Instead of building museums of war," emphasizes Leonard Weinberg, "the Italian parties, in effect, build museums for ideas" (p. 145). Terrorist groups prefer action to museums. Once formed, the various studies emphasize, the groups become self-perpetuating as individual and group psychologies become involved. Robert Clark, for example, points out that ETA (Basque Country and Liberty) attacks in Spain increased in 1980 "during the time when Basques were attaining most of their objectives regarding regional autonomy" (pp. 134–35).

Most of the cases and incidents in the two volumes focus on small groups or individual incidents. Alex Schmid in the Slater and Stohl volume provides a useful reminder that "ethnic and nationalist terrorism is likely to cause greater problems than social-revolutionary, class-oriented terrorism" (p. 59). Abraham Ashkenasi, in the Merkl volume, analyzes Jewish and Kurdish groups. He concludes that success necessitates a "strong social case" as well as paramilitary organization and that the urban guerrilla is "doomed to be ineffectual" without support from the countryside (p. 331).

Both volumes reveal the substantial growth of our scholarly understanding of these various phenomena centering on political violence and the need for further systematic research. Policy perspectives are and will become ever more abundant, given the complexity of the subject matter. Nonetheless, as the former head of Israeli military intelligence has emphasized, technical military solutions won't be discovered. "The solutions are political ones" (Slater and Stohl, p. 231). No cookbook for dealing with low-intensity warfare will emerge from our present preoccupation. But there are intellectual, societal, and governmental reasons for greater understanding of this persistent and perhaps expanding phenomenon.

PAUL WALLACE

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Defining Political Development. By Stephen Chilton. Boulder: Lynne Rienner, 1988. 135p. $17.50.

Stephen Chilton uses Lawrence Kohlberg's Piagetian theory of individual moral development, which elaborates six stages, each characterized by a distinct cognitive structure of moral reasoning. He indicates that there are ways in which individuals relate that are characteristic of each stage and that are necessarily supported by these cognitive structures.

Chilton also offers a "Speculative Classification of Social Forms," which specifies the institutional arrangements for which these ways of relating provide a necessary base of shared expectations. Institutions require appropriate "ways of relating which are publicly common," Chilton's definition of political culture (p. 25). On this foundation he defines political development as "a specific form of change (i.e., advancement to higher Kohlberg states) in the political culture of a society" (p. 28).

Chilton asserts that this definition overcomes several problems. His conception relates individual and institutional systems (the "micro-macro connection"), while the faults such studies as Hagen's, McClelland's, and Almond and Verba's, which identify development with individual attributes, and Huntington's, which consider only institutional change. Chilton's concept of political development effectively proposes a clear locus of development, which is subject to empirical investigation. As he suggests, a number of other authors retreat to discussion of clusters of characteristics that blur distinctions between what development is and its causes, and consequences.

The normative aspect of development is problematic, in part due to the necessity of cross-cultural application. Chilton insists that the issue is nonetheless inescapable: the concept carries an implicit normative connotation. He seeks normative grounding in Kohlberg's stages in two respects. First, the stages specify individual structures of moral reasoning, not content. In parallel fashion, at the societal level Chilton's concept of political development identifies a sequence of cultural organizing structures, not specific social forms: a given stage may include different institutions from different cultural traditions. Thus, the immediate problem of ethnocentricity is resolved in that development is not directly equated with approximating any particular institutions.

Second, Chilton seeks to transfer to the level of political culture the proposition that advancement in Kohlberg's sequence represents individual development because individual cognitive reasoning at higher stages is psychologically more integrated and differentiated. The case that comparable advances in political culture indicate higher levels of political development is less persuasive (that higher stages are normatively better). Alternatively, reference to the institutions that these changes in political culture make possible would be a reversion into ethnocentricity.

While Chilton denies that he is proposing a theory of political development, his discussion of "development dynamics" demonstrates that his concept offers a promising source of insights into how development occurs. His "speculative classification," associating institutional forms with particular stages of political culture, raises many useful questions, for example, To what extent are limits on possible institutional arrangements available to a society defined by the level of political culture of that society as a whole or of the subculture of its elites or that of its opinion leaders? How is the actual functioning of institutions affected by these respective levels of political culture or by the differences among them?

Chilton has made a significant contribution to the study of political development. He offers a creative and aesthetically satisfying conceptualization. Moreover, the interesting and potentially worthwhile research possibilities suggested by his approach make it a valuable working tool.

JOEL C. EDELSTEIN

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The Efficient Secret: The Cabinet and the Development of Political Parties in Victorian England. By Gary Cox. Cambridge: Cambridge University Press, 1987. 187p. $32.50.

The subtitle of this excellent and important book unfortunately gives a false impression. It implies an appeal mainly to the concerns of historians of British institutions in the nineteenth century. But in fact it should interest all students of political institutions, regardless of the culture or era on which they specialize. It provides an elegant model of how to identify crucial features in the development of institutions. And the mode of argument is, in principle, easily transferable to the study of contemporary United States or ancient Athens. Furthermore, since it builds on the research and controversies of some of the earliest of modern political scientists (e.g., A. Lawrence Lowell, Mosei Ostrogorski), it reveals the possibility of cumulative knowledge about relatively evanescent objects of scientific inquiry.

The institution to be explained, "the efficient secret," is the combination of executive and legislative leadership in the cabinet. In the genesis of this combination, the crucial features are the developments, jointly, of party discipline in Parliament and the electorate. The fact of the discipline was, of course, observed as it occurred. The phrase *efficient secret* is from Bagehot (1865). And Lowell in 1902 documented the development of parliamentary party discipline in the latter half of the nineteenth century. But the relation of these facts to electoral politics has, as Cox shows, generally been misunderstood. Following Ostrogorski, the conventional explanation is that democratization forced organized political parties to manage the expanded electorate and that organized parties in turn imposed discipline on Parliament, resulting in cabinet dominance of legislation. So democracy is the root of the "efficient secret." Cox disputes this view, however, and shows that party discipline developed gradually throughout the nineteenth century, reaching its peak in the late 1860s and early 1870s while electoral partisanship developed rather abruptly in the 1860s. Yet democratization occurred only in 1868 and 1885.

Party discipline in Parliament was, Cox shows, practically nonexistent in 1810 when each member was as independent as U.S. legislators of that era. But party cohesion was quite high in 1836, declined somewhat in the 1850s and 1860s, and reached contemporary intensity in the 1870s. What occasioned the change? Certainly not party organization in the electorate, which was not particularly significant until the 1880s—indeed organized electoral parties follow—rather than precede—parliamentary parties. Instead, Cox argues it was the increase of business in Parliament that precluded individual political entrepreneurship. If only because of the constraints of time, the increase of business necessitated procedural reforms that brought party discipline and cabinet dominance. So it was the industrialization and modernization, not democratization, that increased parliamentary business and that then necessitated and induced Parliamentary party discipline.

To study party discipline in the electorate Cox exploits a heretofore-almost-unused resource, namely, the vote counts in double-member districts, which were the main kind of districts prior to 1885. In a double-member district each voter could cast up to two votes, though not two votes for the same candidate. In such a district nonparty voting is revealed as follows. Suppose there are two Conservative and one Liberal candidate. Voters may (1) vote only for one Conservative (nonpartisan plumping, i.e., ignoring the second Conservative), (2) vote for the Liberal (partisan plumping), (3) vote for one Conservative and the Liberal (split voting), and (4) vote for both Conservatives (party voting). Choices 2 and 4 reveal party regularity; choices 1 and 3 reveal indifference to party. From 1818 to 1865 these two kinds of nonpartisanship, that is, 1 and 3, were quite common. After 1865 they were rare.

Why did this fairly abrupt change occur in the 1860s? While Cox sees an influence from idiosyncratic events and from increases in the size of the electorate and number of contested elections, his main answer is that party discipline in the electorate followed directly from party discipline in Parliament. Once a partisan cabinet controlled the Parliamentary party, the only avenue left for citizens to control the direction of public policy was for them to control the selection of the cabinet. And for this party regularity in the electorate was at least advantageous and probably necessary. So the causal chain reaches from modernization leading to parliamentary congestion, leading to parliamentary party discipline, leading to party regularity in the electorate—a quite different story from that of the text books.

Although this is, at base, a case study, it proceeds by statistical generalizations about parliamentary and electoral behavior. Since we have very few such careful studies of the development of an institution, this one is both a model for others and a superior building block for generalization.

WILLIAM H. RIKER

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Rational Ecology: Environment and Political Economy. By John S. Dryzek. New York: Basil Blackwell, 1987. 270p. $29.95.

This book is an inquiry into which modes of societal decision making and problem solving have the greatest potential for addressing an interrelated array of increasingly serious environmental problems. Expressed another way, the challenge is how to achieve a greater measure of ecological rationality where the dictates of economic, social, legal, or political rationality have traditionally prevailed.

Dryzek offers a typology of what he calls "social choice mechanisms," which, he concedes, is not comprehensive, nor are the categories mutually exclusive. At the national or subnational level, he identifies five such mechanisms that have been widely practiced. Western societies tend to rely heavily on markets as an expression of social choice, whereas administered systems are more prevalent in the socialist states that engage in central economic planning. Social choices can come about through political jockeying among competing groups comprising a polyarchy, judgments made in accordance with legal principles, or moral persuasion as powerfully exercised by Mao in China. At the international level, which Dryzek describes as an anarchy, decisions may also be dictated by markets, administered systems, and legal principles or, alternatively, may be the product of bargaining, armed conflict, or coercive diplomacy.

The author concedes that humans inevitably intervene in natural systems but insists that they must not be allowed to destroy the capacity of nature to support them. Thus, ecological rationality is defined in terms of a "symbiotic relationship" in which humans are neither the masters nor the slaves of nature. The potential of the various social choice mechanisms to achieve ecological rationality is assessed in terms of several criteria. First, do they encourage negative feedback that prompts corrective steps when the system veers from its desired course? Second, do they facilitate coordination among actors and between choices on different issues? Third, are they robust and flexible in the sense of performing well under a variety of conditions? Finally, are they resilient in their capacity to steer human and ecological systems toward a more normal range?

Attributing significant shortcomings to the social choice mechanisms that have been widely adapted at the national and international levels, Dryzek introduces three additional possibilities: an open society in which social decisions are subjected to a free flow of information and debate, practical reason based on a logical consideration of the suitability of strategies for achieving a desired end, and radical decentralization à la Schumacher's dictum "small is beautiful." Having assessed these additional alternatives, he finally opts for a mode of social choice that combines a limited form of bargaining with practical reason applied to specific functional areas. To implement his proposal, Dryzek shuns de novo designs for social engineering in favor of what he calls "meta-design"—developing the "structural capacity to engage in innovation and design" (p. 245).

This is a book of broad theoretical brush strokes rather than elaborately developed and substantiated arguments. As such it is a readable, thought-provoking book that offers new insights into some of the questions raised previously by Hardin, Schumacher, Heilbroner, and Ophuls about the type of political system best suited for coping with ecological imperatives. In presenting his theory, Dryzek draws together a diverse body of literature on subjects such as ecology, philosophy of knowledge, political institutions, decision making, public policy, market theory, legal systems, international relations, and social change, and engineering.

The most significant contribution of this book to the literature on political ecology is the assessment of the strengths and shortcomings of the various social choice mechanisms. Dryzek's proposals for an alternative mode of decision making that would achieve a higher degree of ecological rationality lack clarity and are rather brief. The lack of a persuasive solution to the ecological predicament will probably limit the book's impact. Furthermore, the critique of social choice mechanisms at the national or subnational level is much more thorough and authoritative than the assessment of how issues and problems are handled in the international realm. For starters, the phenomenon of international regimes, which in the past decade has become a dominant focus of international organization specialists, is given only passing treatment in a footnote in the introductory chapter. Given the extent to which ecosystems transcend national political boundaries, the imperative for ecological rationality is especially compelling at the international and global levels.

MARVIN S. SOROOS

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Refreshing Pauses: Coca-Cola and Human Rights in Guatemala. By Henry J. Prundt. New York: Praeger, 1987. 269p. $34.95.

A few years ago one of the leaders of the Coca-Cola workers union in Guatemala, Israel Marquez—then in exile in Mexico—told of an assassination attempt. Marquez explained that as he was driving to his home in Guatemala City, a passing vehicle sprayed his car with automatic rifle fire. He fell to the floor as glass shattered above him. When asked what he did next, he laughed and said, "I put the car in the garage." Marquez's sense of humor is characteristic of those union activists in Guatemala who, over the past 30 years, have had to organize in an atmosphere of terror. It's not that they find anything humorous in seeing coworkers murdered by the government but that the ability to laugh reaffirms their own humanity. Frundt's book does an admirable job of conveying the extraordinary courage of the Coca-Cola workers in their attempts to press for better wages, improved working conditions, and the right to organize. Frundt argues that it was this courage, combined with pressure from international, religious, human rights, and labor organizations on the Coca-

Cola Company's headquarters in Atlanta that allowed the Coca-Cola union in Guatemala to win its demands. For Frundt "this case represents one of the first concrete examples of successful collective bargaining across national boundaries" (p. 227). The study also describes this struggle in the context of the larger fight for social justice within Guatemala by examining "how this union was able to survive to become the key stimulant of a resurgent labor movement that in turn brought reconsideration of military rule and more respect for human rights" (p. x).

Frundt, a sociologist, delves into an area that should be the terrain of political science. But there has been very little political science research on Guatemala in recent years. Obviously, research is difficult when your respondents may be killed just for talking with you. And the few studies on the political aspects of Guatemala that do exist are often so filled with the horror of the past 30 years that it is difficult to get a sense of the political dynamics among the various actors of the Guatemalan polity. Combining the documentation available with the testimonies of many of the participants, Frundt has written not only a useful but an extremely interesting book about labor issues in Guatemala and explains them in the broader context of Guatemalan politics.

The book is an intriguing account of a right-wing Texas attorney, John Trotter, who employs death squads to quash attempts at unionization at the Guatemala City Coca-Cola plant, which he owns. Frundt describes the events that forced working class Guatemalans to go into exile and recounts the murder by the Guatemalan government, of labor and political leaders for merely attempting to organize. These are vivid and painful accounts of an extremely violent period in Guatemalan history. While many of the events are familiar, there has been no systematic attempt to put them in order and analyze them. This is one of the strengths of Frundt's book.

The case of Coke union leader Israel Marquez illustrates the events of the Coca-Cola strike that led to the transformation of an ordinary Guatemalan worker into an internationally recognized labor activist. Trotter first attempted to bribe Marquez into leaving Guatemala with an offer of $15 thousand and a visa to the United States. When Marquez rejected the offer, Trotter threatened to have him killed. He survived several attempts and eventually went into hiding. Believing he was still in his home, the death squads killed a couple that had rented the Marquez house. Finally understanding that his assassination was imminent, Marquez took refuge at the Venezuelan Embassy and was given asylum in Costa Rica. Later Marquez embarked on a speaking tour and described the almost unbelievable events of Guatemala to audiences throughout the world and then to a Coca-Cola stockholders meeting. Marquez's transformation is the embodiment of the Coca-Cola union's struggle for democracy in the midst of terror.

The book is a welcome addition to the literature on Guatemalan politics. It compliments James Painter's *False Hope, False Freedom* (1987), which examines the first year of the civilian government of President Vinicio Cerezo as well as the military which remains the main power broker in the country and Michael McClintock's *American Connection* (1985), which provides extensive documentation on the network of terror utilized by the Guatemalan government. Another study worth mentioning is a small pamphlet, *Soft Drink, Hard Labour* (1987), which provides a quick overview of the Coca-Cola strike.

Refreshing Pauses is an important work for those interested in Latin America and essential for those concerned with Guatemala. It will also be useful to scholars looking at linkages involving local labor issues and transnational corporations.

MILTON H. JAMAIL

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Feeding African Cities: Studies in Regional Social History. Edited by Jane I. Guyer. Bloomington: Indiana University Press, 1987. 249p. $29.95.

This edited volume is a collection of empirical studies on the dynamics of food supply and production in four African urban areas—Kano in Nigeria, Yaoundé in Cameroon, Dar es Salaam in Tanzania, and Harare in Zimbabwe. The authors focus on how the African urban population, or what Guyer dubs "permanent" or "specialist" consumers, are fed.

Guyer argues that the current disciplinary models of food supply in urban Africa—the competition-price model, the ethnography-human geography model, and the economic sociology model—separately and singularly are inadequate as plausible explanations of food supply. The neoclassical economic competition-price model aims only "at assessing competition and its effects on prices" (p. 9). The anthropological ethnography-human geography model focuses "on the various forms of social organization which deliver the goods . . . , the 'consumer basket' and how, in terms of logistics, it is filled . . . , how the urban consumer's physical needs are met: sources of food, types of transport fuel, water and housing" (p. 14). The Marxist-based economic-sociology model emphasizes "the determinants of the urban standard of living" and how these "ensure economic welfare and hence political stability" (pp. 17, 18).

A common chord permeates the four studies. They all reveal that there exist observable patterns of differentiation in African social and economic life that work in tandem with external (international) forces that qualitatively and quantitatively dictate the direction of urban food supply. These patterns of social and economic differentiation, Guyer argues, can be best understood and appreciated in terms of the social histories of the various urban areas and their rural peripheries. The social historical analysis engaged in by the authors depicts the complementarity of the three existing models of urban food supply. Thus an integrated paradigm of urban food supply can reasonably and logically be constructed by combining the three. That integrated paradigm or "regional social history approach," according to Guyer, enables us to (1) look at the interrelationship of social and economic life within the total system, (2) understand the development of those administrative organizations and bureaucracies in shifting political and ecological situations, and (3) outline the major contours of a particular urban supply system.

The four cases demonstrate an interconnection among the historical, socioeconomic, and ecological processes—colonialism, world grain and energy conditions, supportive administrative bureaucracies, culture, arability of land, and politics—in developing a more plausible (if not definitive) paradigm of urban Africa food supply: "The social and material process is not entirely dominated by the ethnic factor assumed in the past by anthropologists, the state domination assumed by political scientists, the evolution of a recognizable market system by neo-classical economists, nor the class formation assumed by Marxists. Concentration on particular local areas where all these processes meet is one step towards understanding the organization of this society [for food supply]" (p. 47).

The cases present some interesting insights into the dynamics of food production and consumption using African data. I have doubts about the author's using only four cases to develop this sweeping structural framework of food supply. How generalizable can this framework (the regional social history approach) be in explaining how the many other urban areas are fed, given the fact that there are over 50 countries in Africa? Guyer does recognize the generalizability problem. She concludes,

> The purpose of this collection . . . has not been to develop a new and rigid typology; the cases are too few, their complexity and under-documentation too great. It has rather been to focus attention on the critical need to conceive of the food market in a broader sociological and historical context, and to provoke awareness of the conceptual challenges of describing dynamics in a theoretically useful manner. In substantive terms, the cases serve to illustrate the resilience of the African construction of material life, not as a static quality, but as a product of regional political processes, achieved over a period of extraordinary instability and structural change." (p. 243)

The regional sociohistorical framework articulated forcefully by Guyer must be subjected to the test of time and further inquiry. It would be interesting to see if the study of other African urban areas and their peripheries would support the seemingly deterministic framework generated from the Kano, Yaoundé, Dar es Salaam, and Harare data. Nonetheless, these four cases highlight the inherent limitations of the behavioral (disciplinary) approach and depict the comparatively superior "explanatory power" of the past—behavioral (interdisciplinary) in studying the complex phenomenon of urban food supply. The book is a major contribution to the field of comparative public policy analysis.

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The Soviet Union under Gorbachev. Edited by Martin McCauley. New York: St. Martin's, 1987. 247p. $39.95 cloth, $14.95 paper.

Politics, Work, and Daily Life in the USSR: A Survey of Former Soviet Citizens. Edited by James R. Millar. New York: Cambridge University Press, 1987. 423p. $49.50 cloth, $16.95 paper.

Will the Non-Russians Rebel? State, Ethnicity, and Stability in the USSR. By Alexander J. Motyl. Ithaca: Cornell University Press, 1987. 188p. $24.95.

In light of *glasnost* and *perestroika,* writings on the contemporary Soviet scene run the danger of being overtaken by history. For all three books, readers are likely to ask whether ongoing changes in the Soviet Union are undermining the information, findings, and prognostications contained in these scholarly works. This issue touches on each of the books under review, despite differences with respect to fundamental purposes and potential audiences.

The Soviet Union under Gorbachev contains papers originally presented at a 1986 conference on Gorbachev's first year in office. The purposes of this text are threefold: to examine the legacy of socioeconomic problems inherited by the Gorbachev regime, to describe the various policy options advanced within the Soviet press and scholarly journals, and to assess the prospects for reform in the USSR. The book's strength lies in providing a historical backdrop for current reform efforts.

The anthology contains 11 well-written chapters, each oriented toward key organizational or policy questions. The topics covered include Gorbachev as leader, state and ideology, law and reform, nationalities, the economy, agriculture, foreign trade, labor, motivation and productivity, Eastern Europe, defense and security, and foreign policy. The largely European list of authors is impressive, with outstanding scholars like Philip Hanson, Ronald J. Hill, David Lane, and Karl-Eugen Wadekin among them. As a supplement to a basic text, this edited volume should find favor with those teaching undergraduate courses on Soviet politics.

But *The Soviet Union under Gorbachev* will need to be supplemented with other materials in order to address reform issues adequately. The book focuses on the time frame from Gorbachev's assumption of office as general secretary to the Twenty-seventh Party Congress in February–March 1986. Much has occurred since that date, especially with respect to economic reform. McCauley, for example, notes that Gorbachev "appears to believe that the key reason [for economic decline] was a drop in discipline, order and morality" (p. 31). While this was a commonly held assessment flowing from the earliest days of the Gorbachev era, it is an altogether inadequate interpretation from the perspective of 1988.

The research presented in *Politics, Work, and Daily Life in the USSR* includes the first essays stemming from the Soviet Interview Project (SIP). Like the Harvard Project, which involved interviews with former Soviet citizens after World War II and resulted in a flood of invaluable research findings on Soviet society, SIP promises to provide scholars with a unique resource for probing everyday Soviet life. The Soviet Interview Project surveyed nearly three thousand respondents in the United States, with the bulk of interviews taking place in 1983. The respondents were largely Jewish emigrés who had lived in an urban environment in the Soviet Union. For most of them, their "last normal period" (LNP) of life in the USSR—the five years leading up to applying to emigrate—ended in the late 1970s. Thus the SIP research concentrates on conditions extant during the late Brezhnev period.

Although some information regarding life histories was gathered, survey questions generally focused on the LNP, with questions tapping education, employment, language, ethnicity, religion, household structure, standard of living, bureaucratic encounters, military experience, and sex roles, as well as attitudes toward Soviet institutions and the political system. It should be noted that considerable information is provided in the introductory chapter and in two appendixes on questionnaire development, the selection of both interviewers and interviewees, and the issues inherent to such a difficult undertaking.

The book is divided into four sections. Part 1 ("Introduction") includes an article describing the history of SIP and the problem of bias (Millar) as well as a more substantive essay addressing the issue of Soviet emigrants' subjective perception of the qualify of life during their last normal period of life in the USSR (Millar and Elizabeth Clayton). Part 2 ("Politics: Sources of Regime Support") includes essays focusing on generational change (Donna Bahry), on support for regime norms (Brian D. Silver), and on attitudes toward Soviet science and technology (Linda L. Lubrano). Part 3 ("Work: Economic/Demographic Trends") includes articles on inequalities in earnings and wealth (Aaron Vinokur and Gur Ofer), the labor force participation and childbearing patterns of women (Barbara A. Anderson), and Soviet enterprise operations (Paul R. Gregory). Part 4 ("Life: Social Status, Ethnic Relations, and Mobilized Participation") includes work on perceptions of social status (Michael Swafford), interethnic relations (Rasma Karklins), and patterns of regime-society relations (William Zimmerman).

Although specific percentages derived from a survey of Jewish emigrants cannot be generalized to the Soviet population, the authors build a strong case for viewing patterns as indicative of the urban population's outlook. Some of the findings are striking. In contrast to the Harvard Project, where the young and well-educated were the most supportive of the Soviet system, SIP found a transformation in the structure of support, with the younger and better-educated the "most alienated from some of the fundamental characteristics of the system" (p. 26). At times conclusions merely confirm what Soviet experts have always known (or thought they knew). For example, Swafford's work on social status does show that the working class is not revered, but his research also suggests that the most important determinant of social status is occupation, with party membership, education, and ethnicity also contributing to an individual's prestige. And like the earlier Harvard Project, SIP uncovers certain aspects of Soviet society that generate support. Millar and Clayton note that job satisfaction is strikingly high, and higher for women than men despite the former's generally lower pay and lower prestige. Moreover, substantial proportions of people approve of the Soviet system of medical care, education, and embrace state ownership of heavy industry.

This anthology is a major "event" for the Soviet field and should become required reading for Soviet studies graduate students. Indeed, scholars teaching graduate comparative politics seminars may wish to examine this research for possible classroom adoption. The SIP studies forge bridges between the all-too-often-isolated realm of Sovietology and the disciplines of political science, sociology, and economics.

The third book, Alexander J. Motyl's *Will the Non-Russians Rebel?* focuses largely on one non-Russian nation—the Ukrainians. Motyl argues that while a non-Russian front among ethnic minorities is doubtful, the "Ukrainians—alone of all the non-Russians—have the absolute strength to pose a serious threat to the Soviet Russian state on their own" (p. 50). And in the case of the Ukrainians, Motyl considers rebellion well-nigh impossible. His interpretation of ethnic relationships in the USSR emphasizes the capacity of the Soviet state to smother any incipient uprising.

Motyl's book is of uneven quality. The first three chapters address conceptual problems like the meaning of key terms and the relevance of various models of state-ethnicity relations to the Soviet case. While the author's intentions are laudatory, much of the material seems unrelated to the analyses of the Ukrainian situation that form the heart of the book. Greater conciseness and integration are called for.

In evaluating the Ukrainian situation, Motyl directs attention toward economic well-being, ideology, language policy, and coercion. With respect to material conditions, the author presents evidence that Ukrainians have not "been relegated to the bottom of a cultural division of labor" (p. 66), nor are they "the benighted nation they are often made out to be" (p. 68). But does prosperity necessarily generate acceptance of the status quo? This is an issue that needs further exploration. The fact that both Aristotle and Brezhnev believed it does does not make it always true. Indeed, the SIP research found that those who were reaping the benefits of the system, the young and well-educated, were also the most alienated from the system.

In discussing language policy, Motyl presents an interesting exercise that should be of interest to specialists on Soviet nationality relations. He shows how using different statistical techniques can result in varying conclusions regarding Russian language penetration of the Ukrainian language print environment. Motyl's own assessment of the data is that there is no obvious evidence of Russian language encroachment.

For Motyl, the bulwark of the Soviet state is the pervasive system of control and coercion. It is impossible to rebel in a state that controls communication lines (whether the media or, e.g., the Polish-Ukrainian border), bans unsponsored political activity, and labels the speaking of Ukrainian in many public settings as anti-Soviet. According to Motyl, an intact KGB is the ultimate guarantor of the status quo.

Motyl is writing about a pre*glasnost* Soviet Union. Indeed, the term *glasnost* does not appear in the index. There is, therefore, no discussion of the linkages between reform and nationality issues, a topic of intense interest today. Unsponsored groups are burgeoning and their legal status has not yet been clarified. In official Ukrainian publications Ukrainian writers are voicing complaints about the status of the Ukrainian language that could only have appeared in samizdat a short time ago and may well have resulted in punitive measures. This is not to say that the world Ukrainians live in has changed very much from the one described by Motyl, but an ongoing and persistent reform program could well undermine the conditions he outlines as precluding ethnic rebellion.

These three books are geared toward different audiences and, for the most part, explore divergent themes. Each represents a contribution to the scholarly literature, especially *Politics, Work, and Daily Life in the USSR.*

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Penetrating the International Market: Theoretical Considerations and a Mexican Case Study. By David R. Mares. New York: Columbia University Press, 1987. 294p. $35.00.

This study, the latest in the Columbia University Press series, The Political Economy of International Change, edited by John G. Ruggie, seeks to refine the debate on North-South relations with respect to the issue of structural constraints on southern penetration of northern markets. Building on Ruggie's concept of "embedded liberalism," Mares attempts to develop a general framework for the analysis of the potential for export expansion in international trade that improves upon extant dependency and liberal explanations of barriers to southern trade and entrepreneurship. A detailed study of Mexico's winter vegetable trade with the United States is presented in support of this analytical approach.

Mares's core thesis will not surprise political scientists familair with political economic debates on international trade but may challenge some of the economic persuasion. He argues that neither dependency nor the liberal theory of international trade unmodified by political conditions can provide a sufficient explanation of the nature of barriers to southern trade with the North. Dependency theory offers an unsophisticated and overly restrictive perspective on the prospects for southern penetration of northern markets. While liberal trade theory, in its ideological form, fails to recognize the impact of political pressure on the international trade regime. Ruggie's notion of embedded liberalism, he argues, provides a more sophisticated understanding of international trade realities, recognizing that politics and markets are highly interrelated in the modern world, allowing room for bargaining and manuever by southern trade interests.

Mares attempts to improve on Ruggie's original formulation by arguing that the modern international trade regime is configured as a "segmented liberal" trading regime divided into liberal (free trade) and illiberal (protectionist) segments. Both segments practice protection, he argues, but the liberal segment is distinguished by the playing off and "paying off" of contending interests so that liberalism as a principle and a practice is preserved and extended in international trade. Illiberalism, on the other hand, entails protection "given to producers irrespective of their domestic power in trade policy" (p. 13).

Under the conditions of segmented liberalism, he argues, opportunities exist for increasing southern competitiveness in northern markets. A mature appreciation of this fact requires a multilevel analysis linking local, national, and international economic and political arenas in a multivariate approach to analyzing the structure of international trade, emphasizing (1) product characteristics, (2) the structure of the international market, and (3) the domestic allocation of resources in the producer country.

The bulk of the book consists of Mares's application of this framework to the case of Mexico's winter vegetable trade with the United

States. The trade in winter vegetables provides a useful illustration of the ability of Third World producers to penetrate northern markets, he argues, and one that "is not unique" (p. 5). Growers in the West Mexico state of Sinaloa have managed to build on their special factor endowments for winter vegetable production, the structural importance of exports for the Mexican state, and transnational alliances with U.S. distributors and consumers to capture the leading role among sectoral interests in shaping Mexican domestic and foreign policy on winter vegetable exports. They have effectively managed to reduce their dependency on foreign (U.S.) capital in production and have made substantial inroads toward controlling the distribution of winter vegetables in the United States. In a series of well-researched and illuminating chapters, Mares demonstrates how the Sinaloan growers have managed to succeed in the otherwise risky enterprise of export agriculture.

The relationship between the Sinaloan growers and the Mexican state since the 1930s is of special interest since their influence on the domestic politics of resource allocation is a crucial element in the Mexican winter vegetable story. Relying primarily on archival and interview evidence, Mares shows how Sinaloan growers gained substantial policy autonomy within the structure of state corporatist interest organization affecting Mexican agriculture. The growers' strategic position in the productive process, their ability to create alternate organizations outside the state corporatist system of control and the state's reluctance to intervene directly in this high-risk agricultural sector are combined to enhance the bargaining position of the growers against the Mexican state. Mares's careful research establishes irrefutably the favored position of the growers with respect to the allocation of water, land, and labor costs. His detailed exploration of the political relationships between state agencies, labor, and producers' interests make a significant contribution to our understanding of the mechanics of Mexican corporatism.

The strengths of Mares's study are considerable. Nevertheless, his treatment suffers several shortcomings that can be but briefly addressed here. First, his theoretical discussion of trade regimes is too brief and deserves further refinement. Nowhere is this more apparent than in his rather cursory distinction between regime types. Mares juxtaposes ideological classifications (liberal, illiberal) with empirical constructs (embedded liberalism, segmented liberalism) without clearly specifying the empirical boundaries differentiating these types of regime classifications. As such, he runs the risk of setting out a straw man debate. He would be much better off framing this discussion along the lines of an empirically grounded continuum of the degree of political involvement in trade policy rather than framing his analysis in ideologically grounded distinctions. At the very least this subject deserves further elaboration and greater precision in considering these theoretical distinctions.

Second, while Mares makes a convincing case that Third World countries may penetrate northern markets under certain conditions, that in itself does not add up to a convincing refutation of dependency approaches to this issue. The winter vegetable case may not be "unique," but it is rather exceptional. Mexico itself is an exception among Third World countries by virtue of its level of development and proximity to the United States. While Mares in his concluding chapter finds strong parallels—and support for the utility of his analytical framework—in the cases of Colombian and Korean textile and apparel exports, it is difficult to compare the competitiveness of advanced semiperipheral countries like these to that of Honduras or Chad. Clearly, dependency relations exist in the international trading economy. Just as Mares has chosen to modify and stretch the theory of free trade liberalism to analyze constraints and opportunities facing southern states in international markets, so one can make a substantial case for dependency theory by relaxing some of its more restrictive theoretical assumptions. The most important conclusion to draw here, and one that Mares does make, is that the context of international trade is complex and not easily reduced to ideologically driven explanations.

This is an important study that should be of keen interest to students of international political economy and U.S.-Mexican relations. It raises—if it does not resolve—compelling theoretical questions about the nature of the international trade regime and the opportunities and constraints confronting export-driven development in the Third World. It provides a very useful framework for approaching these questions that recognizes both the necessary linkages among levels of analysis and the complex dynamics—relating products, markets, and domestic resource allocation—that govern export competitiveness. And it is the best study of the U.S.-Mexican winter vegetable deal in print. It deserves a wide audience.

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Elections in Nigeria: A Grassroots Perspective. By William Miles. Boulder: Lynne Rienner, 1988. 168p. $22.00.

Widespread fraud and corruption in Nigeria's August-September 1983 elections, leading to yet another military coup, have been well documented. This book, however, examines the elections and the preceding campaign from a different perspective. Using a journalistic style based on anecdotal materials, the author focuses on one small town, Yardaji, in extreme northern Nigeria. His basic premise is that for civilian governments to have legitimacy in Africa, they must be based on traditional indigenous political culture. Thus, the book focuses on a village that is trying to incorporate the Nigerian second republic's U.S.-style election process into its traditional mindset.

The focus on this tiny, poor, homogeneous Hausa village provides some insights into an area of African politics that is frequently overlooked. Very little research has focused on rural political activity in Africa. The emphasis in the literature is on urban middle-class political action. It is assumed that rural politics is based solely on ethnic and religious factors with little understanding by peasants of national interest and no sophistication about politics. This assumption has been particularly prevalent in the literature about Nigeria, given the civil war and frequent ethnic clashes along with the conservative nature of both civilian and military governments. Miles's thesis, however, is that—at least in Yardaji—peasant politics is not based on ethnicity and religion alone. He examines the wide variety of political activity in the village in the summer of 1983 and concludes that many interests were at work.

The book's greatest success is in providing a flavor for rural politics in Africa reminiscent of Chinua Achebe's *A Man of the People* (1969). Miles successfully analyzes the attempt by modern forces to influence the traditional norms and activities of the village. His examination of the changing role of the Hausa chiefs and their participation in the election campaign is particularly useful. We see a system that is still hierarchical but with modern institutions clearly predominant. With considerable sophistication, however, the chiefs have learned to exert their influence on the modern process in subtle ways.

Miles's examination of the five political parties and their translation of slogans and campaign promises to fit regional differences shows the relative openness of the villagers to outside ideas even if their judgment of the parties is often based on the type and quantity of vehicles at the disposal of party organizers.

Nevertheless, the book has a number of shortcomings. The author's anecdotal style leads him to too many conclusions based on too few interviews. The book's style is also very choppy, with numerous very short chapters. Miles's frequent interspersion of Hausa words and phrases provides a flavor of village life, but the device is far overused.

Most of the author's conclusions about the village as a whole are based on interviews with a few party leaders. Based on the fact that every party was represented in the village by an organizer, Miles concludes that the community was not as homogeneous politically as would be expected. When Miles begins to discuss the actual behavior of the villagers and the candidates, his thesis begins to break down. There is clear distrust and disillusionment with electoral politics to the point where village youth posted a sign at the edge of town warning all parties to stay out. The campaign went on regardless, complete with well-attended campaign rallies that provided much entertainment. The rallies also served to enhance the level of cynicism about the process, however, as they were replete with personal insults and abuses and distribution of considerable sums of money.

Miles's analysis of the election process itself is sketchy at best, partly because of election improprieties and the lack of good data. His analysis shows, however, that all parties did score successes. His findings support the assumption of other authors that official election results in the nation as a whole were fraudulent. The election results for the village would seem to indicate that ethnicity was not the sole determinant. The author does little, however, to investigate other probable determinants of voting behavior.

The book's greatest shortcomings are its failure to place Yardaji in the larger Nigerian setting and, more importantly, the lack of a theoretical context. Both would have strengthened the book considerably.

There is no question that much more work still needs to be done on political activity in rural Africa. Expanded research on village political life would not only enhance the knowledge of social scientists but might also provide some insight into how to adapt modern institutions to fit traditional indigenous norms. *Elections in Nigeria* provides us with a useful taste of village life and provides helpful material on attempts to incorporate democratic practices in a traditional hierarchical society. But one is left with a sense that the book was more of an afterthought than a systematic attempt at research.

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The Dynamics of Working Class Politics: The Labour Movement in Preston 1880–1940. By Michael Savage. Cambridge: Cambridge University Press, 1987. 280p. $39.50.

Labour's Conscience: The Labour Left 1945–51. By Jonathan Schneer. Winchester, MA: Unwin Hyman, 1988. 249p. $45.00.

The Rise and Fall of the Labour Left. Patrick Seyd. New York: St. Martin's, 1987. 236p. $30.00.

The Labour party's defeat in the 1983 general election was crushing. It received just 28% of the vote, a figure so low that one has to go back more than 50 years to find an election when Labour did so poorly. In the 1987 general election the party's performance improved slightly, but the election marked Labour's third consecutive defeat. The last three elections in a row that Labour lost were those of 1951, 1955, and 1959. In 1960 Mark Abrams and Richard Ross dissected this record of futility when they published their ominously titled work, *Must Labour Lose?* But even during this period one could still find signs that the party was vital despite its sickly appearance. One is hard put to find such silver linings today. The contemporary Labour party is in crisis.

So pronounced is this crisis that all three authors see the Labour party's current straits as the context for their books, as a shadow that casts a pall over their work regardless of how far removed from contemporary circumstances their subject may be.

Savage, for instance, examines local Labour politics in the town of Preston in North Lancashire from the party's origin to the Depression. He is particularly interested in showing how the basis of Labour party support shifted from workplace to residential solidarities, from trade union to ward organization. Schneer's book pushes the time frame forward considerably to examine the criticisms and alternatives that the Labour Left offered to Atlee's Labour governments of 1945–51. If, for Savage, the linkage to the Labour party's current crisis is the declining significance of productive relations for party choice, for Schneer it is the road not taken that the Labour Left offered following World War II. Seyd's analysis of Labour politics is the most contemporary of the three and thus touches on the Labour party's current crisis most directly. He analyzes the resurgence of the Labour Left during the 1970s, reaching its high point at the Wembley Special Party Conference in 1981. He then traces the Labour Left's decline in the 1980s, as infighting broke out and the Labour Left failed to shift its focus from reform of the Labour party to winning support outside of it.

The Dynamics of Working Class Politics, by Michael Savage, is better in its component parts than taken as a whole. The first part of the book presents an analytical framework with which to conceptualize working class politics. Savage's effort here is sophisticated and ambitious. He argues against the cultural foundations of working class politics that social historians have developed, and is just as skeptical of political institutions, such as the state or party system, to determine the precise nature of working class politics. Instead, Savage offers an analysis of working class political goals and strategies that is decidedly sociological. He argues that the social structures in which workers are embedded, particularly their skill levels, gender relations, and neighborhoods, generate the potential or capacity for working class collective activity.

The second part of Savage's book seeks to apply this framework to the growth of the Labour party in Preston. Here, Savage argues that the basis of party support from workers shifted as new opportunities for collective action were created by changes in the social structure. Initially, the party expressed the economistic demands of unions in Preston. But as the unions weakened, the party sought an alternative base of support in working class neighborhoods where the capacities for working class activity had improved considerably. As the potential for working class mobilization moved from the workplace to neighborhood organization, what the local Labour party stood for shifted as well. It no longer reflected the economistic grievances of workers concerned about local employment opportunities, but the party now promised state-provided services to tap the new capacity for working class activity based on neighborhoods.

Unfortunately the historical chapters that comprise the second half of the book reveal the limits of the framework presented in the beginning. Savage tends to derive the form working class struggles take—whether they be mutualist, economistic, or statist—from the bias social structures create toward one form of struggle over another. But he fails to appreciate how much ideology—as well as leadership—intervenes between the potential for collective activity generated by social structures and the particular form of struggle that occurs. The perspective is so unremittingly sociological that his history of Labour in Preston is virtually devoid of people—individuals as historical actors, not simply agents. Consequently, the later chapters that cover the rise of Labour in Preston are lifeless.

Savage reaches far in terms of the framework he presents and he probes deep into the local history of Preston to reveal the shifting capacities of working class politics. But in this work of historical sociology, history suffers at the hands of sociology.

Labour's Conscience by Jonathan Schneer is written gracefully and reviews the activities of the Labour Left during the Atlee governments of 1945–51. As we get further from those years and compare Atlee's achievements to the record of Labour governments that were to follow, or to the desiccated state of the party today, they take on added luster. Atlee's contemporaries, however, were not quite so generous. Schneer recounts the criticisms directed at the regime by the Labour Left and its censure of the government as one that lacked both principles and conviction.

Schneer's account of the Labour party during this period is excellent. He demonstrates how the Labour Left came to accept the government's policies which they had criticized previously. With each escalating tick of the cold war—the Marshall Plan, the Czechoslovakian coup, Korea—the Labour Left recast its position. At first it hoped the government would continue to cooperate with the Soviet Union. Then it proposed that Europe, including Eastern Europe, become a third force alternative to the superpowers. Then it gave up more ground to suggest a democratic socialist Europe exclusive of Eastern Europe as a third force until, finally, it adopted the Atlanticism it had criticized initially. All of this is traced in exquisite detail.

The conclusion to the story is Bevan's resignation from the cabinet in 1951 over the proposal to raise defense expenditures and impose charges on the Health Service. But this can no longer be seen as the climax to the Labour Left's gradual disaffection from the government because the Labour Left was moving toward the government's foreign policy views, not away from them. Those who continued to defend the idea of a popular front, or Europe as a third force, were red-baited and intimidated by the party leadership, a story told very well here.

Schneer's sympathy for the Labour Left is evident and it enriches the book. But it also leads him to give more credit to the Labour Left than it deserves. Schneer views each confrontation between the Labour Left and the administration as a crucial turning point for the Atlee government and the future of the Labour party. This overstates the historical weight of a group that had little impact on what the government proposed in the House, was routed at Conference, was poorly represented on the National Executive Council, and was itself only informally organized and beset by fissiparous splits. The Labour Left had to content itself with being the party's conscience because this is the fate of groups that lack power. It raised its voice of conscience frequently because it doubted whether it would be heard otherwise.

The weakness of the Labour Left under Atlee, however, does reveal a fundamental dilemma that confronts any Labour Left. In Atlee's first government, Labour's majority of 146 seats was so large that he could afford to govern without the support of the Labour Left. In his second term, when Labour's majority was reduced to six seats, he could still afford to ignore his Left, certain it would support the government, in fear of bringing it down. The Labour Left did not disappoint him. On the crucial issue that prompted Bevan to resign, the Labour Left voted with the government, fearful that defeat on this question would "lead to an election that a disunited Labour Party would surely lose" (p. 202). In Atlee's first term, when the Labour Left voted its conscience, it was ignored by the government. Now in Atlee's second term, when new arithmetic requires that the government be more solicitous, the Labour Left does not vote its conscience, in fear of the consequences of doing so.

The Labour Left described in Patrick Seyd's *Rise and Fall of the Labour Left* arose in response to the apostacy of the postwar generation covered in Schneer. Harold Wilson, James Callaghan, and Michael Foot were all Labour Left critics of Atlee whose tergiversations as party leader led to the rise of a new, stronger Labour Left 30 years later. Some view it as tragedy, others as justice, that when each new Labour Left arises to condemn the government for lack of conviction, the preceding generation is the target.

The differences between the Labour Left that emerged in the 1970s and that of the preceding generation are profound. Thirty years ago the Labour Left criticized the party's policies. Now it assails the party's procedures and rules. Thirty years ago the Labour Left was composed of members from the Parliamentary Labour party. Now it is less deferential to, and less dominated by, members of Parliament, and its base of support rests on rank-and-file activists in local parties. Thirty years ago the Labour Left confused journalism with political activity and made little effort to organize an internal opposition. Now it stretches party rules against organized internal factions to their limits, and sometimes beyond. Thirty years ago the Labour Left arose full of hope for a Labour government. Now a new Labour Left arises on the ruins of those disappointed dreams.

Seyd attributes the resurgence of the Labour Left in the 1970s to the party's failure in office. The government's policy of wage restraint and its proposals for restrictions on union activities temporarily detached the unions from the party leadership. In addition the party attracted new recruits disappointed with its record in office. Young, educated public employees streamed into the party, determined to reverse its rightward drift. They were joined by women who had been radicalized by the feminist movement, and council house tenants and union members who were outraged by the government's failure to defend their living standards. These groups took over local Labour parties and demanded that the party democratize its procedures. They succeeded in pushing through rule changes regarding the deselection of members of Parliament and the selection of the party leader. They then came within 1% of the vote in electing Tony Benn as deputy leader at the 1981 party conference. Seyd attributes the success of the Labour Left in changing the party's rules and its near success with Tony Benn to its tactical alertness, organization, and leadership. But with Benn's razor-thin defeat the wave crested. Internal divisions emerged, and the Labour Left separated into its traditional "soft" and "hard" left positions and steadily lost influence within the party.

This summary does not do justice to the detail Seyd packs into this short book. It has the familiarity with events and persons typical of an insider—which Seyd is—as well as the judiciousness and perspective one expects of a detached analyst. But the book ends too abruptly. In his conclusion Seyd chastizes the Labour Left for failing to develop a coherent program and for reserving its energies for intraparty disputes when it should have been arguing the case for socialist change with Labour voters and the broader electorate. Unfortunately, Seyd does not even offer a clue as to what the Labour Left's program should be or how it should begin to address a larger audience. His own thinking on these matters does not appear to have gone beyond that of those he criticizes.

Seyd also does not pay enough attention to the Labour Left's credibility gap with the unions. In the run-up to the 1983 election union leaders flirted briefly with the Labour

Left, but this was prompted more by their despair with the party's record than real attraction for the Labour Left. The devastating results of the 1983 general election then drove union leaders reluctantly and without much confidence back into the arms of the party leadership. It would be unfair, however, to attribute the distance that separates the unions from the Labour Left to the cautiousness of union leaders alone. Seyd presents poll data from two local Labour parties that reveals policy differences, especially on foreign affairs, between white-collar and blue-collar Labour party activists. The policy differences Seyd found would probably have been even greater had he compared the white-collar activists he polled with traditional blue-collar Labour party voters. Thus, the caution union leaders feel toward the Labour Left may be a faithful reflection of the reservations their own members have. Unless the Labour Left can allay union doubts, it will be condemned to remain the conscience of the party, a role from which it very much would like to escape.

Lastly, both Schneer and Seyd note in passing the location of Constituency Labour parties (CLPs) supportive of the Labour Left without ever commenting on its consequences for the party. Schneer found that the CLPs most supportive of the Labour Left tended to be in hopeless or marginal districts. Seyd reports that the contemporary situation is still very much as it was two generations ago. Labour Left CLPs tend to be located in constituencies where the party is least competitive. In fact, the predominantly white-collar constituency from which Seyd drew his polling sample of nonmanual Labour activists has returned a Conservative member of Parliament in every election since World War II. The dilemma this poses for the Labour party and the Labour Left is clear. The more the Labour party satisfies and reflects the influence of its Left, the more it would be playing to supporters in constituencies it cannot win at the risk of antagonizing traditional blue-collar voters in constituencies it can. It would draw the Labour party away from its areas of strength and toward these constituencies where it was least competitive. The strategic dilemma this poses is poignant.

Each book reviewed here operates at a different level of analysis in its examination of the Labour party. Savage argues that the party's goals and strategies reflected deep social changes that created different potentialities for working class political activity. Schneer offers a middle ground, arguing that it was the inexorability of the cold war that determined the trajectory of the party. The pull of immediate political events was stronger than the efforts of those who tried to shape them. Seyd's perspective is much more insular. The strategies, tactics, and organizational skill of party factions, he argues, make the difference within it. Only a synthesis of all three levels of analysis—each well represented here—can reveal the shadow that haunts these books, the Labour party's contemporary crises.

ALAN DRAPER

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The Political Economy of Israel. By Ira Sharkansky. New Brunswick, NJ: Transaction Books, 1987. 157p. $29.95.

Sharkansky's account of Israel's political economy is a catalog of information useful to political scientists interested in Israel's macroeconomics. The book provides an extensive account of the determinants shaping Israel's economy. The author takes a refreshing look at the role that bureaucracy and bureaucratic entrepreneurs have in the determination of economic policies and outcomes.

Chapters in the book treat separate yet interrelated topics such as inflation, budgets, center-periphery relationships, bureaucracy and entrepreneurial bureaucrats, standard of living, the state comptroller, and researcher-practitioner relationships.

The author claims to be making a convincing case for the Israeli political economy's being complex, unique, and methodologically problematic. It might well be the case, but the comparative evidence in support of that hypothesis is insufficient.

The descriptive power of the book clearly outweighs its analytical claims. Sharkansky's semiquantitative analysis of Israel's political economy falls short of other related political-economic empirical writings on other Western democracies such as Alt, Beck, Brittan, Chrystal, Fassbender, Frey, Hibbs, Peretz, Tufte, Whiteley, and Woolley to name but a few.

The approach Sharkansky takes might be the probable reason for his labeling of Israeli inflation as a "conundrum," a problem without solution, as he defines it. One reason for inflation's being a such "conundrum" is, according to Sharkansky, its simultaneous status as both a cause and an effect (p. 98), a revelation standard economic literature made long ago.

The present situation in Israel concerning inflation has proven the problem to be soluble. This is due to both good luck (regarding the international economic environment, upon which Israel has almost no impact) and the economic policies that the national unity government has implemented and that won the full support of the Israeli public. The forecast that the pressure of deferred demands "sooner or later will be released into the economy" (p. 91) has so far been void. Israeli inflation in 1988 stands at a steady annual rate of 16%. The author's analysis falls short of capturing the reasons for the sudden control over inflation, partially due to a policy choice made by the unity government.

Another "new" concept Sharkansky proposes, is policy making by indirection. ("What officials cannot achieve directly, they may be able to achieve indirectly" [p. 121]). This is a trivial optimization approach, based on satisficing, bounded rationality elements of policy problems. Indeed, Alt and Chrystal noted in their *Political Economics* (1983) that an interdisciplinary treatment of macroeconomics has to consider optimization under habitual behavioral constraints, human-psychological limitations, and environmental-contextual characteristics. Israel's economic public policy is probably, in this respect, no exception to the rule.

Sharkansky displays various economic data in his confirmation of the "uniqueness" claim. Standard-of-living indicators partially improved, although Israel's national income per capita had declined in relation to other Western countries. The explanation of this phenomenon, according to Sharkansky, lies partially in problems of measurement and a generous external environment (U.S. contributions). Israel's underground—or, rather, "black"—economy, a substantial source of existing measurement difficulties, deserves a more extensive analysis than that provided by Sharkansky. Research steps towards the estimation of the "black" economy's size have been already taken, mostly by economists, in Israel. The black market economy accounts probably for a good portion of the explanation of the fact that governmental and quasi-governmental bodies in Israel spend more than 100% of the country's gross national product (a point Sharkansky repeats in various chapters of the book).

The author's budgetary analysis leads to the conclusion that "sound and fury signifying nothing is an appropriate label for real budget changes in the context of high inflation. Along with price increases of more than 100 percent in four consecutive years, the real levels and distributions of budgetary resources changed little" (p. 73). This conclusion is at odds with existing budgetary analysis of Israel and the finding, for example, that electoral stimulations of the economy have led to real increases in welfare budgets in election years as means of maximizing reelection chances. The semiquantitative univariate analysis is to be identified as the cause of such inconsistencies. Conventional wisdom holds also, namely, that budgetary manipulations work deeper at the micro (agency) level rather than at the macro (ministry) level.

The author takes an implicit Keynesian approach in respect to government-public relationships. This approach partially explains his underestimation of the role the market economy plays in Israel outside the scope and control of Israeli government. The Israeli individual should be seriously considered in any meaningful macroeconomic analysis. His entrepreneurial skills and sophistication work, in many cases, against government economic policies except in cases of perceived crisis. The use of expectational economic theories and a lesser weight on government capability versus the public would have improved the analytical portion of the book.

The book serves as a good descriptive mapping device of forces at work in the Israeli economy. It gives a balanced picture of bureaucratic, economic, and political determinants in the country. The claim for the "uniqueness" of the Israeli system needs a bit more evidence within a more comparative framework, backed by the growing body of literature on the political economies of Western democracies. Little consideration was given to prominent writers on that subject. Explanatory claims are on shaky grounds, due in part to the "soft" techniques the author utilizes. So are the claims for the introduction of new theoretical concepts: conundrum and policy making by indirection. The reader is advised to use the book as an introductory reader on the Israeli political economy.

BARUCH MEVORACH

Tel Aviv University

Patterns of Development in Latin America: Poverty, Repression, and Economic Strategy. By John Sheahan. Princeton: Princeton University Press, 1987. 393p. $47.56 cloth, $12.95 paper.

Poverty, political repression, and external dependence in Latin America are profound problems that "should and can be answered without waiting for either radical or conservative millenia to arrive" (p. 355). Refusing to kneel before such "totalizing" strategies of development, John Sheahan crafts an even-handed analysis of mainstream economic and political strategies for balancing popular preferences for equality against economic imperatives for efficiency.

Poverty, external economic relationships, market forces, and political repression are shaped by common regional factors (part 1), distinctive national responses to these factors (part 2), and the interaction of economic and political relationships (part 3). Sensitive to the strengths and lapses of both economics (with its values of efficiency and productivity) and political science (with its values of democracy and equality), it is Sheahan's undogmatic conviction that effective, balanced strategies really are possible in Latin America. Failed strategies in Latin America have arisen more from imprudent choices than from structural deadlock.

The development problems facing Latin America are sobering. Despite Sheahan's cautious confidence about devising more successful strategies, his readable, thematic, yet comprehensive analysis of Latin America's development conveys a subtle appreciation for the enormity of the task ahead.

In the first half of the book, Sheahan examines poverty; employment and earnings; trade, industrialization, and growth; inflation, deficits, and stabilization; and ownership.

Poverty and destitution continue to stalk the continent, though the general picture is one of long-term improvement in the basic measures of welfare. Capitalism has a mixed record on reducing poverty. In Chile and Peru—both market-driven economies—the lot of the poor, especially the rural poor, has declined; while in Costa Rica and Colombia their condition has generally improved.

Inequality and poverty have been exacerbated by the slow rate at which people have been able to move from jobs with low productivity to those with high productivity and by the rapid growth of the labor force. Economic growth in some countries has left the majority out of the picture because employment opportunities have lagged behind.

Economic strategy has traversed an arduous landscape. Import substitution and the transition away from it were unwisely managed in much of Latin America. A more-balanced strategy has become necessary to propel forward the development of the semiindustrialized economies of Latin America. Sheahan seems to regard this shift as a painful but unavoidable prescription for future development.

Inflation and deficits—both endemic to the Latin American experience—have deep political roots. While excessive demand and structural imbalance are important causes, strains over the transfer of income from rural to urban elites and the clamor for reformist change loom even larger for high inflation and daunting deficits.

Unequal ownership of land perpetuates poverty. Almost any kind of redistribution system, linked to appropriate incentives, would be an improvement over traditional land tenure. In the modern sector ownership is more complex. Multinational corporations are a mixed blessing, helping growth but doing little to reduce inequality or dependence. Public enterprises could be used more appropriately than they are to achieve broader development goals.

While Sheahan's deft exploration of these persistent economic issues deserves careful reading, political scientists may be tempted to go directly to his analysis of political regimes and economic strategy in part 2. Reminiscent of earlier typologies, Sheahan identifies five distinct regimes, four of which use some kind of modernizing strategy: authoritarian-conservative or reactionary regimes (Chile from 1973); socialist or Marxist regimes (Chile under Allende and Cuba under Castro); middle-of-the-road conservative or reformist regimes

(Costa Rica, Mexico, Colombia, Venezuela, and Ecuador from 1979); populist or nonsocialist reformist regimes (Argentina under Perón); and finally the least industrialized, usually conservative or reactionary, regimes (Bolivia, Haiti, and Paraguay, among others).

Sheahan dissects them all, finding some to be unacceptable, others unhelpful, and very few potentially able to balance efficiency and equality in Latin American development. Socialist and populist regimes do not pursue macroeconomic balance because of their disdain for market forces. Yet Sheahan is also skeptical of the authoritarian conservative regimes, which he calls "market authoritarian." A close cousin of O'Donnell's "bureaucratic authoritarian" state, the "market authoritarian" regime uses the power of the state to repress popular demands for greater redistribution of wealth but does not use the power of the state to promulgate statist economic policies, preferring instead to rely upon market forces and economic efficiency.

Almost inevitably Sheahan's analysis brings him to a qualified endorsement of "middle-road market economies." Their modest abilities to balance the imperatives of the market against the imperatives of popular preferences hold out a glimmer of hope for reducing poverty, bolstering democracy, and holding dependency at bay. Mexico, Colombia, and Costa Rica "can scarcely be considered to have achieved the kind of egalitarian, participatory, self determined styles of development most of us would like to see. . . . [But] they have gone a long way through the strains of transition from old-style societies . . . toward urban-industrial economies able to reduce poverty and to respond to more diverse interests without falling into the reaction and the repression that hit Brazil and the Southern Cone. That is not everything but it is a lot" (p. 309).

Sheahan's analysis of development strategies lacks the heat of more highly charged and sweeping remedies for Latin American ills. Many may welcome this, but others may dismiss it as too complacent about the value of the middle road. In his advocacy of the possible, Sheahan may have granted too much to public choice and too little to structure. One thing is for certain: radicals will find little comfort in his conclusion that "nothing inherent in Latin American conditions rules out either democracy or development or reduction of inequity, provided that each society manages to combine requirements for a well-functioning economy with specific measures designed to broaden participation" (p. 324). Sheahan dares to express a "bias for hope," as Albert O. Hirschman has done for so many years.

Latin America has few choices and must choose wisely among those it does have. For example, it must develop in a context of declining but lingering dependence upon its North American neighbor. While the United States can, and sometimes even does, play a constructive role in alleviating poverty, it is hard to do and usually does not succeed, Sheahan concludes. Latin America must look elsewhere or to itself, because the United States is "neither a consistent force for progress . . . nor a monolith bent on domination" (p. 354).

Balance, pragmatism, and hope are themselves not strategies but they are the essential values underlying workable strategies for developing Latin America. In the din of uncertainty and confusion about the political economy of development in Latin America during the 1980s, Sheahan, an economist, offers a calm and sophisticated analysis valuable to both political scientists and economists.

GUY POITRAS

Trinity University

Is Socialism Doomed? The Meaning of Mitterrand. By Daniel Singer. New York: Oxford University Press, 1988. 324p. $24.95.

The only people busier these days than Montmartre artists sketching tourists' portraits are political observers drawing lessons about the Mitterrand "experiment"—the nearly five years between May 1981 and March 1986 during which the Left ruled France for the first time under the Fifth Republic. Much was expected of President Francois Mitterrand and his socialist legislative majority, who promised nothing less than to change the way French people lived. Indeed, this government accomplished much during its first two years, including reforms in social policy, local and regional government, industrial relations, and industrial policy. By 1986, however, the socialists' symbolic rose had wilted, and in legislative elections voters turned once again to the Gaullist-led Right. Most lesson drawers pro-claimed that the Mitterrand experiment had fizzled.

Daniel Singer, the Paris-based European correspondent to the *Nation* and author of studies of the May 1968 "events" in France and the Polish Solidarity movement, joins this fizzle school on its left flank. Mitterrand and the socialists failed, Singer argues, not because their program was too ambitious but because their political vision was too limited. Instead of promoting *autogestion* (self-management), the Left remained statist in approach, legislating in a vacuum and failing to mobilize public opinion or potential support groups. Although enacting many reforms, the socialists abandoned the spirit of their program, "the promise to break with capitalism, to initiate a long march toward a different society" (p. 115). The French case, Singer concludes, demonstrates not that true socialism is doomed (since it was not tried) but that Western European social democracy, having grown content to manage capitalism's crises, has reached a dead end.

Singer develops his case by interweaving context, chronology, and polemic. The context when the Left took power was unpromising for radical reform: international recession, an ideological climate favoring market solutions over state intervention, and a political Left demobilized by the economic crisis and internecine struggles. The socialists thus came to power disarmed ideologically and organizationally. With a sure sense of postwar history, Singer places the Mitterrand-socialist government in both French and European perspective.

The book's core (part 2, entitled "The Road to Surrender") chronicles the Left's early reform wave followed by retreat beginning in 1982, when balancing its budget and trade accounts became the main objective. Bungled attempts to placate disillusioned party activists —for example, the 1984 educational reform debacle—only hastened the government's slide in popularity. While this story has long been known in France, Singer provides a vivid, balanced summary for a general English language audience. Here his on-the-spot observation post serves him well as he provides numerous trenchant and witty vignettes, especially of socialist politicians. Of Laurent Fabius, Mitterrand's second prime minister, Singer writes, "He had the skill, so appreciated among the enarques [graduates of the elite National School of Administration], of being able to plead anything and then its opposite" (p. 190). Mitterrand appears as a man guided more by opportunism than by vision: "To cut the losses in order to remain fit to fight another battle had been the major art of his career" (p. 212). Far from adopting an egalitarian style, Mitterrand, like General de Gaulle, remained attached to ceremony. For example, Mitterrand's only deviation from the general's protocol at weekly Council of Ministers meetings was to allow his ministers to serve themselves a glass of orange juice.

Singer's ultimate goal is not to tell stories but to draw lessons for European socialists. However, his polemics fail to convince in several respects. One obvious problem with the "Mitterrand failed" school is that the man has proved to be far more unsinkable than the *Rainbow Warrior* (the Greenpeace vessel blown up by intelligence operatives in 1985). Recently reelected to a second seven-year term, Mitterrand II will indeed fight other battles—a displeasing prospect, perhaps, to many, including Singer, but hardly indicative of failure.

Even granting that the socialists failed to fulfill their promise, the question remains of whether another path, a "true" socialist program, was possible. Singer's argument presents three problems. First, while it is true that the Mitterrand-socialist government never attempted popular mobilization (whatever this would have meant), it is hardly obvious that such attempts would have worked, given the weakness, division, and demobilization of such groups as the labor unions. Moreover, successful mass mobilization, as Singer realizes, would have forced the government to practice "permanent brinksmanship" with domestic and foreign opponents, thus running the risk of economic collapse and electoral defeat. Given the fierce international financial pressures the socialists faced in 1982-83 after only a mild reflation, one wonders whether brinksmanship under such conditions would have guaranteed political suicide. Finally, Singer's own New Left-style proposals (especially in chapter 12 ("Socialism and National Frontiers") and chapter 15 ("Socialism to Be 'Reinvented' ") are far too vague to inform any government's agenda.

The Mitterrand "experiment" indeed disappointed many observers, and Singer has provided a biting critique from the Left. But the ultimate verdict on Mitterrand must await the last act, for the enigmatic leader who has been called both "The Florentine" and "Uncle"—the schemer and the benevolent buddy—still commands the stage.

W. RAND SMITH

Lake Forest College

The Political Economy of Modern South Africa. By Alf Stadler. New York: St. Martin's, 1987. 197p. $35.00.

The Political Economy of Modern South Africa purports to identify and examine the kinds of constraints that would need to be "transcended in order for the significant changes to be accomplished" (p. 4). The book suggests that the decolonization of Mozambique, Angola, and Zimbabwe transformed the South African regional political and economic system by polarizing the contending forces into the coercive, repressive, and cooperative powers (of the state) versus greater determination and militancy on the part of blacks, particularly the black working force. Dramatic economic decline in the 1970s coupled with the polarization of contending forces ushered the South African political economy into greater entropy.

It is against this backdrop that Stadler delineates the three significant features of the South African political economy that form the basis of the apartheid state. First he discusses the extent to which the political economy, unlike the sub-Saharan African context, is controlled by "a domestic political class, by the presence of a local bourgeoisie, and by the increasingly intimate links that developed between the bourgeoisie and the state" (p. 22). He buttresses this position by showing that domestic investments by 1980 accounted for over 90% of capital formation.

Second, the close affinity between the state and dominant economic classes reveals a political economy with state corporatism tendencies. Stadler shows how the state provides "capital, technical assistance, and subsidies to producers and [controls] the production and distribution of agricultural commodities" (p. 23). Furthermore, the symbiosis between state and capital shows the state coercing and regulating the supply of labor. Such is the dialectic of the political economy of apartheid where free market principles are often set aside to allow for the exercise of authoritarian controls to buttress an anachronistic social order.

Third, the cold war, East-West axis allows South Africa to be politically and economically classified as among the procapitalistic, anticommunist elements of the international community. The political economy of apartheid is thus fraught with contradictions enabling South Africa to thwart external and internal pressures for change.

In several chapters (5–8) of his study Stadler examines the subterranean logic undergirding the implementation of a convoluted and contradictory set of policies and strategies, the purpose of which is to guide "reform from above" as a reactive process "aimed at controlling, containing, diverting and redirecting the pressures for change emanating from above" (p. 8). Stadler correctly sees this effort and other piecemeal and peaceful reform strategy being supported by liberals and the business community as stillborn, for they all lead to greater repression and exploitation, not greater equality and freedom. He sees them as being a part of "a defensive strategy designed to avert fundamental change, by a process of incremental modification to the status quo" (p. 5).

Stadler's study is a useful insight into the substructure of the South African political economy, how it evolved, and how it sustains itself with the aid of the manifest contradictions of apartheid. If the study has shortcomings, it is Stadler's failure to develop a theoretical perspective on the political economy of South Africa or to test alternative theoretical propositions relating to the direction and survivability of the system. They remain outside of Stadler's interest, or he may have purposely left that chore to his readers. In that regard, Michael Barrat Brown's *Models in Political Economy* (1984) provides an excellent reference, with its extensive contending theoretical perspectives and models.

The book, however, remains an unusually stimulating contribution to our understanding both of what significant historical factors led to the South African apartheid political economy and of the salient features of the contradictions in the system.

ALEXANDER ARTHUR

Tennessee State University

Book Reviews: Comparative Politics

The Challenge to European Industrial Policy: Impacts of Redirected Military Spending. By Bernard Udis. Boulder: Westview, 1987. 154p. $23.00 paper.

Udis's most recent foray into the economics of arms production extends and updates his 1978 study, *From Guns to Butter* (1978). In his earlier work, Udis asserts, he found "few relevant European examples of mechanisms to aid effectively the transition of military productive capacity to civilian output" (p. 1). The question is of importance, given the "economic and social strains which might result from a substantial reduction in the level of military expenditures" (p. 1) or from cyclical variations in such expenditures. Firms that produce for the military suffer unusual difficulties in adjusting to drops in military orders because of the peculiar logic of the market for military goods. It is a market in which intensive product-specific efforts to produce a small volume of goods are more prized than standardization and economies of scale. Unfortunately, the current survey of arms industries in seven Western European countries (France, Germany, Great Britain, Sweden, and, more briefly, Belgium, Italy, and the Netherlands reveals continued complacency concerning the likelihood of such reductions, military expenditures' appearing as "a normal part of the government budget and likely to remain that way" (p. 2). Only in Sweden does the author find a deliberate research effort to address the problem.

Nonetheless, Udis documents a number of intriguing developments that have tended to decrease the dependency of certain European firms and industrial sectors on military purchases, such as the tendency toward greater international collaboration in the development of new products or new technologies or the increased effort to develop civilian uses for military goods or technologies. A subject of preoccupation, of course, is the continued reliance of firms in a number of European nations on export markets for their products. As for industrial policy, it plays an ambiguous role according to Udis. On occasion it has fostered strategies and investments that have decreased dependency on military purchases. On other occasions military purchases have been used to assure the viability of firms that have difficulty competing on the civilian market. Finally, the study reveals considerable national variation in policy approach to arms industries, as well as examples of policy incoherence.

In many ways, Udis's study illustrates the unfortunate consequences of the bifurcation of our two disciplines, politics and economics. Although the book is the work of an economist, the question he asks is relevant to a number of hotly debated topics in our own discipline: What is the driving force behind weapons procurement? Does the quest for economic and social security fuel the arms industry more than the quest for military security? Is there a military-industrial complex? Not only does Udis choose to ignore such questions, but he offers very little in the way of either argument or evidence that could even contribute to the debate. This is unfortunate. The economist's contribution is potentially great, given his power to elucidate economic decisions by industrial firms. This is, however, a power that Udis exploits more effectively in his 1978 volume. In the current work, despite a theoretical introduction that makes an interesting attempt to apply the concept of "economies of scope" to the arms industry, the author spends more time on industrial policy than on the adaptive strategies of firms. The result is a study in which Udis disregards the opportunity to make a unique contribution to the study of military spending and chooses instead to provide the reader with rather insubstantial descriptions of national industrial policies. Though volumes of provocative research on the politics of industrial policy have been published in recent years, this literature is almost entirely ignored by the author. The chapter on France, for example, offers only a superficial overview of French policy and contains no references to the works of John Zysman (though he is cited in the earlier volume), Suzanne Berger, Ezra Suleiman, Jack Hayward, Peter Hall, or others. One thus comprehends the innocence of the chapter's conclusion, in which the author advances that industrial diversification in France may be hampered by the educational system: "While a number of French graduates of the Grandes Ecoles acquire master's degrees at top American universities, it may be difficult for them to start their own businesses when they return home. In an environment which discourages risk taking, it may be easier and safer simply to join a large established French firm" (p. 50). The passage contains no falsehoods, nor does it contain any revelations. It merely ends the chapter with an observation that one generally finds at the beginning of studies that focus on the politics of French industrial policy.

Nevertheless, the questions that lurk beneath the surface of Udis's study are important ones, and his research is potentially relevant to the debates that those questions have provoked. But read *From Guns to Butter* first.

MICHAEL LORIAUX

Northwestern University

Reversal of Development in Argentina: Postwar Counterrevolutionary Policies and Their Structural Consequences. By Carlos H. Waisman. Princeton: Princeton University Press, 1987. 329p. $40.00 cloth, $14.50 paper.

Carlos Waisman has written a book that demonstrates a mastery of the comparative literature of development as well as a superb understanding of Argentine history. He asks difficult questions and answers them straightforwardly. The book is a provocative appraisal of Argentina's economic stagnation and political impasse. It focuses on the reasons for its "reversal" in economic and political fortunes beginning with the military coup of 1943, through the late war years, and into the postwar period and the election of Juan Perón in 1946. In attempting to unlock the question of culpability regarding Argentine stagnation, Waisman has produced a book critical to our understanding of that country. Though I disagree with the conclusions, I admire Waisman's methodology and presentation. Its unwavering thesis makes the book a watershed in the literature on Argentina.

Waisman examines the question of why Argentina failed to become an industrial democracy and traces its demise to policies and institutions established during World War II. He argues that high levels of economic and social development and an expanding liberal democracy were confronted by the depression and war. Before then Argentina resembled the "lands of recent settlement" such as the United States, Canada, and Australia—countries characterized by high land-labor ratios and the absence of large labor reserves, making them prone to "efficient capitalism, social egalitarianism and liberal democracy" (p. 254). However, Argentina departed from the "new country" type in two respects: it had greater concentration of landownership (similar to the rest of Latin America), and (as in the case of the United States) most of its immigrants came from countries other than the mother country (Spain). Given that background, why does Argentina resemble an underdeveloped nation? The decline begins when the Argentine government undertakes radical protectionism for local industry oriented to the domestic market and an inclusionary corporatism aimed at a growing labor force. Thus Argentina developed a self-defeating economic system, in which a large share of the surplus generated by its competitive agricultural sector was allocated to its noncompetitive industrial sector. The 1943 junta and the Perón regime subjectively chose policies that implied an inordinate fear of communism and an inverse Marxism that perceived the working class as potentially revolutionary but co-optable. The heretofore societal domination by the agrarian elite and the state sector's suspicion of a dangerous immigrant working class, based on European models, created the panic that resulted in the political success of Peronism. This unleashed a powerful labor movement that set the stage for subsequent bouts of praetorian politics among military, agrarian, industrial, and labor sectors. The reversals that begin under Perón culminate in the Latinamericanization of Argentina, represented by a "sluggish economy, an inefficient manufacturing sector [and] social blockage" (p. 77).

Waisman writes that the state sector under the aegis of Perón plus the New Right that emerged in the 1930s (counterposed to the oligarchic, conservative Right), took advantage of the spectre of the Spanish civil war to promote a variant of the Italian fascist model adapted to Argentine conditions. He sees the Peronist experience as counterrevolutionary and counterproductive. A European pastiche—unnecessary in the Argentine context—it replaced an evolving liberal democratic experience.

The strength of Waisman's effort is that its propositions are so clearly argued that it invites an intellectual dialogue. I agree with Waisman that there has been a reversal in Argentine development, but I see a darker, more flawed liberalism before Perón and I think the bulk of economic regression, political repression, and administrative mismanagement occurred after Perón. The pre-Perón period, particularly in the 1930s, contained fraudulent elections, an extremely restricted electorate, no competitive mass political parties to which workers gave their allegiance, and a society largely dominated by an uncompromising agrarian oligarchy. For me the Argentine reversal begins not in the 1940s but when the import substitution phase is exhausted and Perón (about 1952–53) and those that followed him attempted to mount a heavy industrial base. It was a world dominated by U.S. capital and West European and Japanese postwar economic recovery, where the terms of trade were already unfavorably disposed to such developments and where capital importation seemed a necessity. I don't think that liberal democracy and dynamic capitalism were secure in Argentina before Perón and that Perón's industrialization policies were such a poor economic alternative. Had Perón not initiated light industrialization, Argentine post-Perón dependency would have been more severe.

Moreover, it is possible to see the Perón reforms not as counterrevolutionary but rather as a spasmotic, populist-driven capitalist economic revolution that implemented sociopolitical, cultural, and labor policies that survived the regime. Perón's import-substituting industrialization made Argentina's working class the best-paid, best-fed, best-housed, and best-clothed in Latin America. Defending these achievements continues, since 1955 and the fall of Perón, to be among the basic ingredients of social struggle, the main cause of the political instability that Waisman sees initiated with Peronism. I think that Argentina's oligarchic-driven liberalism would have been a far worse alternative than an interrupted Peronism. There were no authentic industrializing national development models at the time, Perón's experiment was innovative. It was not a political aberration that undermined a perfectible liberal democracy. I think it is difficult to sustain that Peronism was unnecessary because the revolutionary threats of the 1920s and 1930s that accosted Italy and Spain were not replicated in Argentina. Rather Peronism was a much milder form of *working class–based corporatism,* since the threat was also much weaker, as Waisman argues. The fact that Argentina did differ from the "open space countries" in the extent of its oligarchic landed dominance made an attempted state-bourgeois-worker revolution (Peronism) necessary. Waisman minimizes the authentic *economic interests* that new Argentine indistrialists and the new migrant and old immigrant working class shared along with the social issues agenda of the New Right, Catholic church and sectors of the armed forces. Like any benchmark, the 1946 election deeply divided Argentine society. The triumphant coalition grew in intensity and popularity, and the experiement was ended by force. Does not the Peronist experience make it more likely—rather than less—that Argentina will eventually become more like the democracies of Spain and Italy than like its brethren nations of Latin America?

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INTERNATIONAL RELATIONS

The Cold War Begins in Asia: American East Asian Policy and the Fall of the Japanese Empire. By Marc S. Gallicchio. New York: Columbia University Press, 1988. 188p. $25.00.

Decision against War: Eisenhower and Dien Bien Phu, 1954. By Melanie Billings-Yun. New York: Columbia University Press, 1988. 199p. $25.00.

Cold Warriors: Eisenhower's Generation and American Foreign Policy. By H. W. Brands, Jr. New York: Columbia University Press, 1988. 252p. $30.00.

These three contributions by young scholars are the latest additions to the excellent Contemporary American History series published under the editorial direction of William Leuchtenburg. All focus on aspects of U.S. foreign policy since World War II, with intervention in Asia a major motif. Perhaps the most important theme they share is a concern with evaluating how decision makers reconciled "means" with "ends." More precisely, they examine what constrained both the Truman and Eisenhower administrations from using force in a number of regional confrontations.

Gallicchio's study of the end of the Pacific War demonstrates how, as Japan neared collapse, U.S. policy makers transferred their attention from the task of defeating the Axis to that of containing the Soviet Union and revolutionary nationalism. He particularly highlights the tensions that emerged between coalitions of civilian policy makers in Washington and their military counterparts. Pressure to meet perceived Soviet challenges (such as the movement of Soviet troops into Manchuria and northern Korea or of such supposed proxies as the Chinese Communists and Viet Minh guerrillas in Indochina) increasingly drove U.S. policy. Yet while the State Department and presidential advisers pressed to deploy U.S. forces widely in northeast Asia, uniformed officials were keenly aware of their limited and diminishing assets once Japan surrendered. Washington's suspicions and desire to challenge Moscow were, in the end, mitigated by generals and admirals who saw Asia as simply too vast an arena for a test of wills.

Billings-Yun's *Decision against War* is a model of clarity. Written with great elegance, the author turns a case study into a deft examination of President Eisenhower's "style" of leadership and how a talented executive can play off and direct willful subordinates with their own agendas. Although the book avoids saying much about how and why the United States defined Indochina as strategically vital, it shows quite vividly how Ike (and Army Chief of Staff Matthew Ridgeway) recognized the futility of intervention to rescue the doomed French garrison at Dien Bien Phu. Determined not to waste lives and treasure on meaningless military gestures, the president manipulated Secretary of State John Foster Dulles, Joint Chiefs of Staff chairman Admiral Arthur Radford, Congress, and the British into erecting barriers that made U.S. intervention unlikely. This was a deft feat, given the desire of so many in the administration and Congress to prevent the "loss" of Asian real estate to communism. The issue was complicated by Washington's desire for the French to support German rearmament and by the drama of the "Army-McCarthy hearings" unfolding at the same time as the Indochina crisis. Billings-Yun is careful to note that Eisenhower, like his "hard line" advisers, still clung to a determination to halt the advance of communism in Southeast Asia. But Ike defined the challenge as one of constructing a viable regime in southern Vietnam once the French had been defeated on the battlefield and had left the United States a free hand. In fact, many in Washington were not particularly upset by France's humiliation. Now the "can do" Yankees would save the day.

H. W. Brands draws many of these themes together in his collective biography of eight key members of Eisenhower's foreign policy team (John Foster Dulles, Milton Eisenhower, Allen Dulles, Walter Bedell Smith, Robert Murphy, C. D. Jackson, Harold Stassen, and Henry Cabot Lodge). The group included career professionals, anticommunist agitators, and self-promoting gadflys. Like the president they served, all evinced both "a mixture of almost unthinkable anti-communist ideology" with a reluctance to translate that ideology into action. Above all else, Eisenhower's firm hand kept the group in check. Ike, like many in his generation, saw Stalin and the Soviet Union as similar to Hitler and Nazi Germany. This evil must be opposed and contained but, Eisenhower realized, could not be stamped out. The balance of military forces in the world made a World War II-like victory over the Soviets impossible: "an uneasy truce was the most he expected."

Still, Ike and his footsoldiers were determined to enforce the truce with tactical assaults on communist salients in the Third World. Thus, the president supported strongly the development of a powerful covert warfare arm within the CIA. He both approved and championed operations in Latin America, the Middle East and Africa. Given the nuclear balance, the cold war would be fought on the periphery, not the center, of the opposing blocs.

While none of these books contain startling "revelations" or unearth critical documents, they enrich our understanding of U.S. foreign policy by recreating the mental worlds in which policy makers hatched their plots and held their breath.

MICHAEL SCHALLER

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The Coal of El Cerrejón: Dependent Bargaining and Colombian Policy-Making. By Harvey F. Kline. University Park, PA: Pennsylvania State University Press, 1987. 216p. $23.50.

El Cerrejón, a $1.8-billion coal export operation run by Exxon in northeast Colombia, is the largest natural resource project brought to completion (outside of oil) in the last decade. Using concepts and concerns from the *dependencia* perspective, Harvey F. Kline joins the ranks of a careful, sophisticated generation of analysts that includes David Becker, Richard Doner, Joseph Grieco, Michael Shafer and Richard Sklar. *El Cerrejón* is low in generalities, high in detail, and careful in the use of fact and theory. It would appeal to the Fernando Henrique Cardoso of 1989 more than the Fernando Henrique Cardoso of 1969. But Fernando Henrique would wish, I believe, the book and its successors to offer even more penetrating, critical analysis.

Kline devises a spectrum of Colombian bargaining positions that runs from *nationalist*, which favors coal development with no involvement by foreign investors, to *reactionary*, which agrees to coal development in whatever form the transnationals desire. He rates the principal 1982 investment contract between Exxon and the Colombian government in which the two split the investment costs with the former managing the operations, as a disappointing 6, the lowest grade on the nationalistic side of the scale.

What prevented the country, Kline asks, from pursuing a more assertive outcome? The technology of open-pit mining, using electric shovels, dumptrucks, and a railroad are not so esoteric as to demand direct foreign participation. Why not be a nationalistic 10 and do away with the foreigners altogether (except, perhaps, for foreign construction firms to help launch the project)? One reason, he suggests, was a shortage of capital. But a more fundamental explanation, he argues, lies in the "inferiority-complex mentality" (p. 179) of key government leaders. Rejecting a unitary rational actor model, he contrasts the "near-reactionary" bargainers who "genuinely believed that cooperation with the TNEs [transnational enterprises] was to the benefit of the country" with younger technocratic "near-nationalistic" bargainers who would have favored a parastatal Colombian enterprise with minimum outside participation. The near-reactionaries, alas, won out.

In today's world is this such a bad outcome? The price of coal has dropped by almost 40%, utilities are slashing demand, independent suppliers are bearing the brunt of the cutbacks in contracts. A new Colombian state enterprise of the kind the "true nationalists" wanted, burdened with twice as much debt borrowed at high interest rates, trying to move steadily increasing volumes of coal onto world markets on its own, would be a disaster. Better to have Exxon, with deep financial pockets and established buyer-seller ties, to help weather the current period while Colombia continues to collect a 15% royalty on each ton the foreign producer ships. The near-reactionaries seem to have achieved a wily nationalistic victory of sorts.

The point is not to expect Kline or the Columbian technocrats to have had a perfect vision of weak energy prices that hindsight now offers. The key point is for analysts and bargainers alike to define a "nationalistic" outcome pragmatically in terms of what benefits a self-interested country under diverse conditions of supply and demand and of who bears the risk if conditions turn sour, rather than merely (despite Kline's disclaimers) in terms of the *form* of the agreement.

The last 10 pages of the book open up a subject that could help set the agenda for the next generation of *dependencia* scholars. Kline asks whether Colombia might not be able to change the terms of the El Cerrejón agreement in the future. To answer this question, one must examine the way the project was financed. Since Exxon used its own funds for its share of El Cerrejón, the company may find itself held hostage to its fixed investment and fall easy prey to the "obsolescing bargain" when conditions improve. But Exxon's willingness to bear all the risk for its portion of the sunk capital makes the El Cerrejón case atypical. With increasing frequency today multinational resource investors are using project finance to create a transnational alliance of lenders dedicated to reinforcing the stability of the original contract. Recent petroleum agreements in Colombia (and mineral agreements elsewhere), for example, open up the possibility that international financial institutions including the World Bank could suspend disbursements on all loans to Colombia if the country were to raise agreed-on tax rates on the existing operations of Chevron, Occidental, or Elf Aquitaine too sharply. Indeed special new B-category loans bunch the World Bank's lending in the later maturities when the foreign investor is most vulnerable. The challenge for future researchers is not merely to document how multinationals demand favorable initial conditions for their investments but rather to uncover how they are devising strategies and techniques to parry the thrusts of nationalistic young technocrats as the projects evolve over time.

THEODORE H. MORAN

Georgetown University

Terrible beyond Endurance? The Foreign Policy of State Terrorism. Edited by Michael Stohl and George A. Lopez. Westport, CT: Greenwood, 1988. 368p. $49.95.

Terrorism: The Cuban Connection. By Roger W. Fontaine. Philadelphia: Taylor & Francis, 1988. 206p. $29.95 cloth, $14.95 paper.

Terrible beyond Endurance? is the third volume of its kind, since Stohl and Lopez previously edited *The State As Terrorist* (1984) and *Government Violence and Repression* (1986). The premise of these collections is that governments use terrorism much more extensively than the nonstate organizations that have captured the public's imagination and concentrated the minds of Western governments, especially the United States. The focus in the volume at hand is "state behavior in the international arena"—including coercive diplomacy; clandestine, state-sponsored, and surrogate terrorism; and acquiescence in the terrorism of others (p. 4). As this listing shows, the scope of the volume is not restrictive. Although the multiple issues of state repression, use of deceptive violence as an instrument of foreign policy, and deliberate attacks on civilians during warfare are important subjects that merit scholarly attention, the research effort is hindered by inadequate definition of precisely what is to be explained. "Coercive terrorist diplomacy," for example, includes all state actions designed to make noncompliance with demands painful. "State-sponsored terrorism" refers to "the destabilization of other societies *with the purpose of creating chaos and the* conditions for the collapse of governments, the weakening of the national states, and changes in leadership" (pp. 4–5).

The chapters in this volume are consequently wide-ranging in approach and in content. The volume is introduced by Charles Kegley, Vance Sturgeon, and Eugene Wittkopf, who contend that the structure of the international system condones and encourages terrorism because nuclear weapons and the strategy of deterrence create an underlying "terror syndrome." The second section of the book ostensibly concerns the United States and terrorism, but it contains a chapter by Leonard Asimow and Frederic Homer on Israeli treatment of Jewish underground terrorism in 1984, as a study of the general problem of how democracies might acquiesce in terrorism within their own societies. The analysis then turns in detail to U.S. interventions in Latin America and support for repressive regimes in the Third World, including Guatemala in particular. U.S. policy in these areas is regarded as consistently subversive of democracy and dismissive of human rights.

The third section of the volume deals with "regions and state terrorism." It includes an analysis of the ASEAN and Gulf Cooperation Council nations and their regional security systems, which are designed to respond to both internal and external threats. There is a chapter on the Israeli invasion of Lebanon, considered by Ilan Peleg as "counterterror terrorism." Peleg sees the 1982 invasion as a result of the failure of Begin's counterterror campaign, itself an organic part of Israel's coercive diplomacy in the Middle East. George Quester then considers the general problem of state-supported terrorism in the Middle East. He suggests that terrorism is ingrained in Arab political life, in part because lines between domestic and foreign policies are often blurred. The next chapter seems more relevant to the concerns of the second section, since it involves U.S. arms transfers to the Third World. Deborah J. Gerner concludes that although the United States readily supplies repressive technology to countries with the worst human rights records, there is no evidence that without U.S. assistance the general incidence of state repression, or terrorism against domestic society, would be reduced. South Africa is the next object of consideration. The chapter focuses on the nonaggression pact with Mozambique as a means of exporting state terror, defined apparently as military intervention, covert operations, economic destabilization, and coercive diplomacy. The volume concludes with a study of the laws of war governing military intervention and humanitarian conduct in warfare. It uses the My Lai incident to reveal the ineffectiveness of international law in limiting violence against civilians.

Roger W. Fontaine intends to shake U.S. complacency about terrorism from another political angle. In the Stohl and Lopez collection, there is no specific treatment of any of the states considered by the U.S. government to be responsible for international terrorism in the 1980s: Libya, Iran, Syria, and Soviet Union, Cuba, or South Yemen. The United States is considered the main culprit. Fontaine, however, singles out the Castro regime for having "steadily expanded its scope for terrorism to a nearly world-wide basis" (p. 1). Although when one tries to ascertain precisely which Cuban agencies are involved in what activities, "much remains a mystery" (p. 10). Cuba is assumed to work closely with the Soviet Union—in fact, to serve its interests—and also with Libya and the PLO. Fontaine describes the gradual development of Cuban support for terrorism and the wide range of Cuban-sponsored revolutionary activities in the Caribbean, South America, the Middle East, sub-Saharan Africa, Puerto Rico, and the United States. He concludes that terrorism will continue to be a major instrument of Cuban foreign policy and that the United States is unlikely to do anything about it, since the Iran-Contra affair has paralyzed the counterterrorism program. The book also contains about 50 pages of documents (reprints of statements from Latin American revolutionary groups) and photographs mostly of captured weapons.

What can be concluded from this review? Certainly terrorism, however defined and in whatever form, remains a central topic for inquiry. Since it is so broadly conceived, there are always aspects of it that appear not to have been studied. Often one suspects that this is because the events in question simply were not called terrorism. It also remains a highly politicized and contentious subject. Stohl and Lopez deserve full marks for their care in applying standards of evidence and for their desire to see that a full range of approaches to the study of terrorism be represented in the literature. The magnitude of the power that can be exerted by governments far exceeds the destructiveness of antistate terrorism. However, the neglect of interventionist states other than the United States, South Africa, and Israel is unfortunate. The opinions that motivate Fontaine's book are predictably transparent, as would be expected from a journalist for the *Washington Times* from 1983 to 1987 and a member of the National Security Council staff from 1981 to 1983. His model is Claire Sterling's *The Terror Network* (1985) and his purpose is to prove that "Fidel Castro's contribution to the 20th century" is "inhumanity . . . disguised by a web of deceit" (p. 147).

MARTHA CRENSHAW

Wesleyan University

Game Theory and National Security. By Steven J. Brams and D. Marc Kilgour. New York: Basil Blackwell, 1988. 199p. $49.95 cloth, $19.95 paper.

Steven Brams (a political scientist) and Marc Kilgour (a mathematician) have collected in this book several of their recent papers applying game theory to various analytical problems in the study of international conflict. Included in the book are discussions of arms races, deterrence, the problem of terminating wars and other conflicts, the Reagan administration's so-called Star Wars program, optimal threats, crisis stability, the verification of arms agreements, and the probability of nuclear war.

All the papers analyze variants of the same game, which the authors call the conflict game, whose structure is specified in the introduction. By varying the payoffs and other features of this game, the authors try to adapt it to fit all the various analytical problems discussed in the book.

In addition to the distinctive features of this generic conflict game, the authors claim that their analysis is distinguished from most game-theoretic discussions of international conflict by being more rigorous. Instead of using simple games as metaphors for problems in international relations, the authors claim they have rigorously derived conclusions about the issues discussed in the book by solving the games they use to represent them.

This book does indeed place greater emphasis on solving games than many ostensibly game-theoretic discussions of international conflict; thus game theory plays a greater role in the derivation of its conclusions. The games discussed in the book are complicated enough to have nonobvious implications but simple enough to solve without extreme difficulty. Thus they lend themselves to use as exercises in introductory courses on applied game theory. Moreover, the authors emphasize mixed strategy as well as pure strategy equilibria.

Unfortunately, Brams and Kilgour place greater emphasis on solving the games discussed in the book than on finding the best game-theoretic models for the issues to which the book is devoted. The conflict game is a simple modification of chicken and the prisoner's dilemma. As in these familiar games, two players must independently choose between two alternatives. But in the Brams-Kilgour modification, if one chooses *C* while the other chooses not-*C*, the player who has chosen *C* is allowed to "retaliate" by altering his choice before the payoffs are allocated. This basic conflict game is then further altered for use in various contexts. For example, by assuming a continuum of strategies ranging from *C* to not-*C*, players are allowed to choose among levels of cooperation. All the games discussed assume complete information.

Brams and Kilgour make virtually no effort to show that any of the problems they discuss are best modeled by games with rules such as these. Since (as this book usefully illustrates) the solutions to games are highly sensitive to the rules that define them, it is not clear that the book is anything more than a series of interesting and challenging exercises in game theory. Moreover, some of their discussion of the substantive issues to which the book is ostensibly addressed seems odd. For example, they assume that "the heart of the problem with deterrence, especially of the nuclear kind, is the apparent irrationality of retaliating against a first strike by an opponent" (p. 43). If that were true, there would be little point to the controversy about whether U.S. nuclear weapons can be used to deter a Soviet conventional attack on Western Europe.

In fact, the main motivation for the definition of the conflict game seems to be to investigate the effect of allowing each player in chicken or the prisoner's dilemma to respond to the choices made by the other. This is an advance over naive efforts to convert these simple games into a theory of international politics. But Brams and Kilgour offer no compelling reason to think that this modification of them moves us much closer to that goal. But the disparity between the intuitive way these issues are usually discussed and the models presented in this book is not inherent in game theory itself. For, as the authors say, theoretical progress requires that national security analysts try "to formulate the rules of different games that seem to capture salient strategic features they wish to model," solve them, and then compare the solutions with the behavior they seek to explain (pp. 174–75). This book is a useful example of how to do that, and it challenges anyone dissatisfied with the result to specify game rules that better capture an intuitive understanding of the problems.

R. HARRISON WAGNER

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Antarctica and South American Geopolitics: Frozen Lebensraum. By Jack Child. New York: Praeger, 1988. 322p. $39.95.

Child's readable and important book on the possibilities for conflict in or around Antarctica among South American nations contains four central themes. First, "Antarctic policies and actions of the key Southern Cone nations have a strong geopolitical tone and . . . it is possible to provide explanations, and perhaps even predictions, for these policies and actions by analyzing the available geopolitical literature" (p. 187). Such geopolitics, asserts Child, aims for access to or possession of Antarctic territories and adjacent maritime spaces that would build national development and moral regeneration through attainment of strategic projection; mineral, energy, and food resources; and even ecological and meteorological extension from the polar areas to South America.

Second, most of these policies toward Antarctica can be characterized as conflictual, since South American geopolitical thinking follows an organic thesis that posits a natural struggle for space and resources in a chaotic and hostile world. The Brazilian frontage theory, for instance, of polar ownership through projection of South American coastal boundaries southwardly along unobstructed meridians to the South Pole, has tended to destabilize regional geopolitics, pitting Chile and Argentina, who are normally antagonists, against Brazil, Peru, Ecuador, and Uruguay. Third, Child can visualize some reason for a "cautious optimism" that previous regional stances toward Antarctica could now be shifting to a cooperative attitude of Latin American integration and solidarity. His judgment for this is based on recent trends of Southern Cone democratization, the Argentine Falklands (Malvinas) debacle, and Brazil's decision to press for Southern Cone integration. Fourth, if this South American solidarity should transpire in either a joint Southern Cone Antarctic Quadrant contained within the 0°–90° west meridians or in a revised frontage design, another set of difficulties could arise: heated protest by non-Latin nations who would be excluded from such an Antarctic sector.

In dealing with these four themes, Child describes in some depth traditional geopolitics of Argentina, Chile, Brazil, and other republics as they pertain to Antarctica, and he concludes by drawing an extensive matrix outlining possible geopoligical outcomes: conflictual, cooperative, and mixed.

Four other sources in English cover some of the author's same territory: César Caviedes' *Southern Cone* (1984), Howard Pittman's "Geopolitics in the ABC Countries" (Ph.D. diss., 1981), Philip Kelly and Jack Child's *Geopolitics of the Southern Cone and Antarctica* (1988), and Child's *Geopolitics and Conflict in South America Quarrels among Neighbors* (1985). Yet none of these focus as exclusively on the geopolitics of Antarctica as the present book, and none contain the central theses examined above. The Child book is an original and insightful vision of geopolitical dynamics in the Southern Cone area that should be the primary source on this topic for years to come.

The value of the book stems from several perspectives. It is clearly written and the organizational format is particularly helpful in moving from general to specific information on national geopolitics, then back to a more comprehensive conclusion. The data are accurate and well integrated into the four central themes. Valuable also are the sources. Most are by Latin American writers infrequently cited in English works. Child's description of geopolitics is measured, positive, not value-laden, and not apologetic. Indeed, his overall depiction of geopolitics as such contributes to the 1980s resurrection of the term as a valid theoretical approach to the study of international affairs. Finally, the importance of Antarctica is not overstated. It is a region of likely future importance, not of immediate global consequence.

Two problems need mention also. I wonder if in his fourth theme above Child exaggerates the impact Southern Cone republics exert on Antarctic outcomes. It is difficult for me to imagine a significant change at present in the polar regime—certainly any change induced exclusively by the South Americans. And perhaps in this point lies an additional factor as well. Antarctica is an intriguing laboratory for a study of international conflict and cooperation, but its location, remote and inhospitable, lacks the immediate importance to draw the attention of Great Power statesmen intent on dealing with the primary problems of the present world.

PHILIP KELLY

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Trade Wars: The Theory and Practice of International Commercial Rivalry. By John A. C. Conybeare. New York: Columbia University Press, 1987. 319p. $35.00.

Trade disputes are important manifestations of international conflict in a domain characterized by the benefits of exchange. Why they arise and how they are resolved pose important challenges to international relations theory and international political economy.

In this important book, Conybeare, who treats nations as actors interested in maximizing national income, combines game theory and international trade theory to generate three hypotheses linking relative size and bilateral trade wars. His first argument is that trade conflicts should not arise between small states because such nations most prefer mutual cooperation and can only hurt themselves by imposing tariffs, whether unilaterally or in retaliation. In an asymmetric relationship between a large and a small power, on the other hand, the large country has a dominant strategy of defection, whereas the small one has a dominant strategy of cooperation. Trade wars are instigated by the exploited smaller power, which ultimately capitulates. Finally, the relationship between large states is marked by the existence of a prisoner's dilemma in which strategies of contingent retaliation induce cooperation. Trade wars between large countries should be resolved with iteration. Conybeare recognizes that iteration is less effective when there are more than two large states engaged in a trade dispute, but he argues that nations will quickly act to bilateralize the problem.

With this theoretical structure, Conybeare elegantly analyzes trade relations from the ancient to the modern world. The historical discussion is wide ranging and impressive. In one chapter, a beautiful refutation of Polanyi and Finley, he demonstrates that the complementary nature of Greek trade—rather than economic primitivism—explains the absence of trade wars in the ancient world. In still other chapters, Conybeare demolishes the theory of hegemonic stability. He argues that large countries prefer predatory optimal tariffs to free trade and prefer to bilateralize trade negotiations and minimize the degree of publicness in trade policy. (He is aware that his argument of hegemonic predation leaves him the problem of explaining British policy in the mid-nineteenth century and U.S. policy in the mid-twentieth century). Throughout, Conybeare provides provocative interpretations. He argues, for example, that the 1934 Reciprocal Trade Agreements Act represented not a shift from the 1930 Smoot-Hawley tariff but the pursuit by new means of the same predatory strategy by the United States.

I do find Conybeare's definition of trade wars—as involving the use of restrictions on the free flow of goods and services for economic objectives—to be insufficiently precise. Although this definition allows him to ignore economic barriers established for political purposes and so to distinguish the U.S. trade conflict with Japan from U.S. limitations on trade with the Soviet Union, a problem remains. It is not clear how the use of such constraints during a trade war differs from their use during other times. Virtually all states employ some trade barriers, yet conflicts flare up only periodically. States engage in trade even as they limit exchange in one way or another. Typically, the restraints are accepted, gains from trade are still realized, and some interdependence develops despite the restrictions. Disputes arise when states change their system of barriers in order to restructure the terms of trade. These conflicts involve escalating trade barriers in the movement from one mutually acceptable framework for international exchanges to another, and Conybeare's definition does not fully capture this.

Moreover, Conybeare's models cannot explain the initial shift from an equilibrium relationship, the very change that begins a trade war. His models all have but one equilibrium outcome. Thus, although he can explain both the absence of departures from equilibrium (i.e., why small states do not fight trade wars) and returns to equilibrium (i.e., why iteration eventually leads large states to return to mutual cooperation), he cannot explain departures from it. He cannot tell us why mutually advantageous relations are nurtured and then disrupted. His analysis invariably requires that either the original trade link or the trade war reflect real interests and be an equilibrium outcome and that the other be a departure from equilibrium that reflects misperception or domestic rent-seeking not in the national interest.

The author is admirably sensitive to the importance of plausible rival hypotheses. He shows, for example, how domestic rent-seeking is a viable alternative to relative economic size as an explanation for foreign economic policy. Ironically, however, because he also uses rent-seeking to explain deviations from expectations derived from his model, one finishes the book sensing that the case for domestic rent-seeking as an explanation for foreign economic policy is as strong as his primary argument on behalf of a unitary actor model positing that nations seek to maximize national income.

This is a rich and erudite work. It is incisive and insightful. It demonstrates a broad command of international political economy, international trade theory, positive theories of politics like the new economics of regulation and domestic rent-seeking, and relevant economic histories covering more than two millennia. It is a book that rewards careful reading. It is a necessary touchstone for subsequent discussion of these issues.

ARTHUR A. STEIN

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The Politics of Economic Interdependence. By Edmund Dell. New York: St. Martin's, 1987. 268p. $37.50.

This is an excellent work by an experienced practitioner of political economic statecraft who served as secretary of state for trade in the Wilson government. The author is a liberal pragmatist who believes that the primacy of a state's concern for national economic security should be balanced by concern for individual freedom and personal welfare.

Dell holds that states seek to preserve national economic security even in a world of increased interdependence. Each state's ability to preserve its economic security depends on the political constraints under which it operates and the strength of its economy. Negotiation, reciprocity, and accommodation will occur as long as participant's interests are served.

Coordination of economic policy by states undertaking negotiated reciprocity is the best available means to ensure international economic growth but will never be easy because each state seeks to preserve its own economic security. The failure of efforts to coordinate world economic policy by reciprocity fosters protectionism. Dell asserts that the international economic system will collapse if the United States succumbs to domestic pressures and pursues protectionism. Emphasis now must be on settlement of disputes through negotiation. The principle of international discussion and the reconciliation of states' national economic security interests must be preserved.

A major focus of the book is a perspective critique of classical liberal arguments for free trade. Dell is not a protectionist but believes that states will take protective measures to preserve their economic security because power will not easily be sacrificed to liberal economic doctrines. He asserts that while economic theorists may be interested in freedom of trade, states are interested in protection of their national economic interests. Protective measures need not always lead to protectionism. Paradoxically, economic liberalism has led to the new protectionism, because liberals have neglected to realize the state's responsibility to maintain its national economic security. For example, tariff reductions under the General Agreement on Trade and Tariffs (GATT) have resulted in sophisticated forms of protectionism. In Dell's opinion the incremental reduction of tariffs might have been more reasonable, whereby the more sophisticated forms of protectionism might have been avoided.

The day of resolving economic conflict by military force is past. State economic power is increasingly important in influencing negotiation among states. Dell points out that both Germany and Japan have gained more by economic efficiency than by war: "A new power structure is being created in the world, one based not on military power but on economic power and technological efficiency" (p. 9). The old international system based on military power continues to exist along with the new power system based on economic resources. While the military power of the United States has not been challenged by Germany and Japan, its domestic market has been invaded. The instinct for national economic security requires states to retain control of their economy and their independence in policy formulation even in an era of increasing interdependence: "Interdependence converts economic security into an explicit tenet of policy" (p. 18). Of particular relevance to the United States, in view of the increasing domestic pressures for protective measures, is Dell's point that while economic interests can misuse the economic security argument to secure protective measures, all protective measures should not be viewed as acts of official capitulation to interest group pressure.

The author presents a fine analysis of the limits of economic interdependence and international economic commitments. Interdependence will never control the search for national security. The problem with policy coordination is that it inevitably runs into states' self-interest in economic security. Interdependence enables states to impose costs on another state, but in the end each state will act on its own security interest and seek to avoid imposed costs of economic interdependence. The rules of such international organizations as GATT are inoperable and unenforceable when they threaten a state's economic security.

Dell makes incisive comments on what he calls the "war of the worlds," that is, the conflict between developed and developing countries. Too much time is wasted on poorly conceived proposals from developing countries and the best weapon at the disposal of developing countries is the sense of guilt of developed countries. Free trade is not always the most feasible option for developed and developing countries. While reduced protection of domestic markets has brought benefits to the "four tigers" (Hong Kong, Taiwan, South Korea, and Singapore), it may not provide miracles in other countries. Indeed, the degree to which liberalization has contributed to the miracles of the newly industrialized countries is not clear.

Dell includes a discussion of the new mercantilism, which is guided by the principle of protection of national economic interests and acceptance of the role of the state in protecting these interests. The new mercantilism is not any more protectionist than the free-trader's adjustment policy exceptions. A modern mercantilist accepts a role for the state in economic affairs and the fact that political considerations influence economic policy.

The author concludes that the integration of the world economy has been based on reciprocity and negotiations rather than on adherence to free trade principles. Governments fear the costs of free trade and regard the benefits as unproven or occurring in the future. Political constraints and economic doubts mean that states will continue to pursue reciprocity with the option always open to revert to restrictive practices.

Dell has provided a thought-provoking treatise. The work is perceptive and judicious in its judgments regarding free trade and the new mercantilism and provides valuable insights into the current debate in the United States regarding the domestic budget-trade deficit nexus and into global economic relationships.

ALBERT J. OSSMAN, JR.

Bethany College

Balance of Payments Adjustment, 1945 to 1986: The IMF Experience. By Margaret Garritsen de Vries. Washington: International Monetary Fund, 1987. 336p. $14.50 paper.

Margaret Garritsen de Vries worked for the International Monetary Fund (IMF) from its inception in 1946 until 1987. This survey of 40 years of experience with balance-of-payments adjustment draws on her previous official history, but treats some topics in greater depth. The chapters are organized chronologically, with separate treatments for the adjustment problems of industrial and "developing" members within each time period.

The first chapter is the most interesting, for in summarizing the hopes invested in the IMF at its founding the author gives a hint of how profoundly the actual evolution of the fund and the world economy has betrayed those expectations. The objective of public officials at the end of World War II was "to simultaneously attain and maintain internal equilibrium (full employment) and external equilibrium (balance of payments equilibrium)" (p. 9). Full employment was the primary goal of national policies at that time, and J. M. Keynes fought doggedly to defend the freedom of debtor countries to engage in expansionary domestic policies: "The Bretton Woods system was designed to provide stability of exchange rates without the restrictions on money supply imposed by the gold standard" (p. 10). Current transactions could be controlled during the transition period. Capital controls could be used for persistent imbalances. Balance-of-payments adjustment would be achieved through a combination of controls, drawings from the fund to permit the maintenance of imports, and infrequent changes in exchange values.

Within a few years fund officials, following the lead of the most powerful fund member, the United States, had delegitimized controls on both current and capital account as a tool of stabilization. There is heavy irony in the fact that "the Fund regarded the introduction of external convertibility as the most signal achievement of the period after World War II" (p. 31) but felt that "the liberal provisions of the transition arrangements helped foster an excessive and prolonged reliance on restrictions" (p. 35). The truth is that the Bretton

Woods system worked well only in the "transition period" and began to fall apart as soon as external convertibility was restored by the major Western European countries at the end of 1958—the point at which the par value system was supposed to come into its own. When controls on capital were removed, international money movements began to destabilize currency values, leading to the demise of the fixed rate system in 1971-73 (chap. 4).

The dismantling of controls created a gap in the armory of weapons for dealing with balance-of-payments disequilibrium. The gap was filled by a revival of the use of monetary policy "following its disuse in the 1940s when . . . Keynesian economists attributed the prolongation of the depression of the 1930s at least in part to the weakness of monetary policy" (p. 37).

Monetary policy may have been willingly revived by the capitalist governments of industrial countries but in the fund's response to Third World members it legitimized the sort of internal meddling Keynes had striven to prevent. Full employment was gradually abandoned as a goal (see pp. 90, 125). The fund emphasized economic growth through exports and devalued internal growth achieved by reflation because the latter required some use of controls to dampen import demand.

Some important errors of judgment on the part of fund management are admitted by the author: the optimism in late 1971 that the payments imbalances of industrial members would be shortlived (p. 93) and the insufficiency of exchange rate changes proposed by the fund at that time. De Vries finds what she calls "a rather comforting explanation: it was not that the fund's MERM model was wrong but that the size of the 1971 disequilibrium had been seriously underestimated" (p. 124).

The most serious error was the decision in early 1974 to accommodate the oil shock by stressing financing rather than adjustment for the Third World, so that industrial members could adjust *their* economies by maintaining and even increasing exports to the "developing" countries. The fund's own contribution to recycling, the oil facility, was dropped after two years because industrial members felt it competed with their private banks' business (p. 132). And while the fund gave lip service to increasing "adjustment" after 1976, the conditions it required tended to increase the need for financing—and thus debts—because import and exchange controls were dismantled. The fund throughout the late 1970s followed the United States by issuing overoptimistic assessments that world growth would soom improve and that heavy Third World deficits could be financed indefinitely by private capital.

The outbreak of the debt crisis in 1982 should have shaken this optimism, but the myths persisted that growth would solve the problem and private banks would resume voluntary lending—because these myths saved the banks from the devastating consequences of immediate default. De Vries describes the Baker plan of 1985 as enshrining "a continued central role for the fund in conjunction with increased and more effective structural adjustment lending by the multilateral development banks," whereas most observers saw in the plan a tacit admission of the extreme unpopularity of the fund with its Third World members.

The record of the fund in 1986, the end of the period covered by this book, was clearly one of failure to control payments imbalances. The commercial banks' decision in 1987 to increase loan-loss reserves for Third World loans signaled the ineffectiveness of the debt crisis strategy championed by the fund (at the behest of the United States) for five years. And payments imbalances among the industrial countries—U.S. deficit, Japanese and German surpluses—were worse than at any time since the inauguration of the fund, with no solution in sight.

This is a discreet and reserved volume that does not even touch upon the most famous scandals surrounding the IMF, such as its long and supportive relationship with Marcos of the Philippines and Mobutu of Zaire. It is based chiefly on articles previously published by IMF staff members and on the interpretation of internal documents not available to outside researchers. It is thus a semiofficial collective memoir, with the advantages and drawbacks of that genre, an authoritative but biased and selective history. With these limitations, it will be of interest to specialists but difficult for beginners to interpret for both technical and political reasons.

CHERYL PAYER

New York

International Sanctions in Contemporary Perspective. By Margaret P. Doxey. New York: St. Martin's, 1987. 175p. $37.50.

Margaret Doxey has been studying problems of collective economic sanctions and international enforcement for 20 years. In her latest book, she once again provides concise summaries of the major case studies and offers insightful assessments of their meaning. She expands on her earlier work by examining recent experience with "voluntary, *ad hoc*" sanctioning efforts, as well as those authorized by international institutions such as the United Nations. In addition, Doxey draws together research from other sources that highlights the importance of identifying unstated, but nevertheless important, goals and *all* potential costs, including those to the "sender," third countries, and the economic system as a whole.

Doxey begins her analysis by distinguishing economic *sanctions* ("penalties linked to real or alleged misconduct") from broader uses of economic leverage for narrow, national purposes (p. 4). As one who has been guilty of using the term rather loosely, I believe the distinction is a useful one. Doxey also differentiates economic from diplomatic and cultural sanctions, noting that a major reason the former are preferred is that they are expected to have "coercive, as well as demonstrative and punitive" effects.

However, the conventional wisdom regarding economic sanctions is that they "never work." Moreover, they impose costs, sometimes quite high, on the countries imposing them, as well as on the targeted country. Hufbauer, Schott, and Elliott's *Economic Sanctions Reconsidered* (1985) was the first attempt to analyze these costs systematically, and they concluded that the cost to the "sender" is a significant variable in determining the effectiveness of a sanctions effort. So why do nations resort so frequently to economic measures, and how may their effectiveness be judged?

David Baldwin's *Economic Statecraft* (1985) pointed out the importance of recognizing that there may be multiple targets and more than one goal and that effectiveness must be judged against the whole range of possible objectives. Sanctions may be imposed more often for symbolic and demonstrative purposes than for coercive or punitive reasons and may be directed at domestic or third-country audiences as much as at the ostensible target. Insights from these and other studies, as well as her own analysis of the recent trend toward "decentralized and fragmented sanctioning" have led Doxey to modify earlier conclusions regarding the role of economic sanctions in international enforcement.

In her 1980 book, *Economic Sanctions and International Enforcement*, Doxey focused on "target-related" goals and based judgments about effectiveness on the ability of sanctions to deter, punish, and—perhaps—end "illegal" behavior. Her analysis of the evidence led her to conclude that the obstacles to collective action—inability to agree on sanctionable violations and unwillingness to bear costs—make the "effective" use of sanctions unlikely even though the potential costs to all parties may be quite high. Therefore, she offers a strong cautionary note against using coercion in international enforcement and recommends placing more emphasis on cooperative problem solving and conflict avoidance.

Although the obstacles to collective action remain, the new book looks more closely at possible unstated motives for imposing sanctions, while the recommendations seek to increase effectiveness while minimizing costs. The recognition that there may be more than one goal means that assessments should be based on the "true" goal, whether or not publicly expressed. Thus, if the primary objective is to send a signal, "economic impact on the target is not crucial, and may not even be particularly important" (p. 144).

In addition, "ethical and prudential considerations are relevant to the imposition of economic sanctions which *are* intended to have a significant impact" (emphasis in original, p. 145). Ethical concerns require that nations consider whether wreaking economic havoc on an impoverished developing country is an appropriate response even when some response is merited. Where the burdens will fall and potential spillover effects on third countries also warrant consideration. Prudence argues for carefully analyzing possible unintended costs and consequences, before sanctions are imposed and when particular measures are chosen. The most damaging costs for countries frivously imposing sanctions may be political rather than economic: signals may be misread and a bad situation made worse. This is a clear message of the recent U.S. sanctions against Panama.

Though disagreements will continue to rage over the utility of economic sanctions, it is clear that they will continue to be used. Therefore, Doxey is right to emphasize the importance of recognizing the multiplicity of goals, especially unpublicized ones, and of carefully analyzing all the potential costs of sanctions, not just those imposed on the target. Whether from an international enforcement perspective or even from a narrow, nationalist viewpoint, it simply makes sense to tailor sanctions more carefully to the objective they are genuinely intended to achieve.

KIMBERLY ANN ELLIOTT

Institute for International Economics

The Grand Strategy of the United States in Latin America. By Tom J. Farer. New Brunswick: Transaction Books, 1988. 294p. $39.95.

Finding Our Way? Toward Maturity in U.S.-Latin American Relations. By Howard J. Wiarda. Washington: American Enterprise Institute for Public Policy Research, 1987. 286p. $27.50 cloth, $16.95 paper.

The Reagan policy toward Central America has spawned a small growth industry in academic (and nonacadĕmic) writing. Two of the major recurrent questions the literature addresses are, To what extent has the Reagan policy represented a substantial shift from that of previous administrations? and To what extent has the administration succeeded in determining the boundaries within which future foreign policy debates and policy implementation will take shape?

The books by Tom J. Farer and Howard J. Wiarda have concerns that go beyond the administration's conduct in Central America. Yet that conduct is in may ways the central motif of their volumes. I will focus primarily on the materials in the books that bear directly or indirectly upon U.S. conduct in Central America. In so doing I unavoidably and unjustly neglect the wider scope of concerns in each book, particularly Farer's.

A professor of law at and former president of the University of New Mexico and a member of the Inter-American Commission on Human Rights (1976–83) and its president from 1980–82, Farer has written a book which contains a decade of essays (many previously published) on international law, U.S. foreign policy, and Third World societies. The four sections of the book – Part 1, "Strategy, Morality, and the National Interest;" Part 2, "Human Rights;" Part 3, "Law and the Practice of Intervention;" and Part 4, "The United States in Central America: Cosa Nostra"—are thematically tied together by Farer's agnosticism toward U.S. efforts to help "reform" the Third World, and the need for restraint in U.S. foreign policy.

A corollary theme of Farer's book is his concern with the continuous debates since the Second World War between realists and idealists, and conservatives and liberals over the proper roles that morality and law should play in the shaping of U.S. foreign policy. He argues that from the beginning of the Cold War until the Carter administration, the United States had a lucid and coherent policy toward the Third World, one that gradually dissipated U.S. influence and poisoned U.S. domestic politics. The policy was characterized by a view of the Third World as an arena of unremitting competition between the United States and the Soviet Union, and of Third World governments and people as pawns on the global chessboard. The Carter administration marked a watershed in that it brought advocates of a consistent, alternative approach into the center of power. The alternative approach sprang from a set of neorealist assumptions at odds with those that had guided global containment. The United States, with few exceptions, would no longer equate its interests with the survival of particular regimes, would be cooler to regimes that grossly violated human rights, would establish relations with leftist regimes willing to practice nonalignment on strategic issues, and would treat Third World states as legitimate and significant negotiating partners in the management of international system.

Farer traces the essential failure of liberal policy toward the Third World to the practical difficulty stemming from the liberal tradition of managing essentially domestic conflicts within Third World societies. The countries on which the United States can exert its greatest influence are in most cases those countries most closely aligned with it on East-West issues, whose elites are committed to such alignment as a means of preserving a structure of privilege often incompatible with the moral dimensions of U.S. policy. Conservative opposition to Carter's handling of foreign policy, Farer argues, united around the two grand themes of the need to sustain friendly regimes against all comers and the need to back diplomacy with conspicuous threats of force.

Wiarda is a resident scholar at the American Enterprise Institute and director of its Center for Hemispheric Studies. *Finding Our Way* is a collection of his earlier papers, articles, and essays, essentially covering the period from the early 1980s through 1986—a large number of them published elsewhere.

Wiarda reasonably contends that early Reagan foreign policy was a reaction to that of the Carter administration, and that Reagan was determined to reverse its main thrusts. This involved deemphasizing human rights, hanging tough on North-South negotiations, and reemphasizing the East-West dimensions of policy. But moderating forces (foreign policy professionals, the press and public opinion, a learning process) tempered the administration's rhetoric as well as its actual policy, and pushed it back toward center.

Wiarda approves Reagan's return "to the historic bedrock of U.S. policy toward Central America. . . . He restored U.S. leadership in the Caribbean and Central America, he reasserted the primacy of U.S. strategic purposes in the region, and he renewed the emphasis on having safe, stable, friendly nations to the south" (p. 260). This realist resurgence combined with an emphasis on democratic development, democracy, and the protection of human rights as means to help accomplish these goals. In sum, Wiarda finds a firmer hand on U.S. policy since 1983 and a coherent, rational, more sophisticated and more discriminating strategy for carrying it out. The prospect for a mature, sounder relationship with Latin America has evolved: "Now, if the Congress can stop playing politics with the issue (unlikely) and if we can proceed with the agenda the [Kissinger] Commission set forth, which, while not perfect, is the best possible in the present context and on which virtually all of political Washington is agreed, then an even more coherent and sustained policy for Latin America may evolve" (p. 6).

Farer and Wiarda share a common perception of the unique way that Latin Americans "do politics," and both remain skeptical of U.S. efforts to force wholesale reforms on that region. Yet they diverge widely otherwise.

Wiarda provides a superficial and dubious justification for policies that have cost billions of dollars, that have poisoned the nature of U.S. domestic politics, and that have extracted a tremendous toll on the physical and social fabric of the countries of Central America.

The nuanced and well-crafted essays by Farer demonstrate that the liberal position on U.S. foreign policy need not have crumbled so rapidly and so completely under the polemical onslaught of the Reagan administration. The collection is wide ranging, penetrating, and thoughtful and seldom engages in facile generalizations.

Yet one senses that as the cold war configuration irrevocably crumbles—internal restructuring in Eastern Europe, military collaboration between the United States and China, a tentative opening of channels between South Korea and the socialist bloc, and so on—both the liberal position (always a pillar of the cold war consensus) and the conservative one retain dubious relevance for the making of policy toward Third World societies. Similarly, the germaneness of the realist and idealist debate is called into question.

ROBERT DASH

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Oil and Mexican Foreign Policy. By George W. Grayson. Pittsburgh: University of Pittsburgh Press, 1988. 207p. $19.95.

"Mexico," President López Portillo was reported to have said, at the height of the oil boom, "is not an oil well." The lesson to be learned by the foreign analyst from this truth is that not everything about Mexico and U.S.-Mexican relations has to do with Mexico's oil policies. Some questions about Mexico and U.S.-Mexican relations are independent of oil prices, for instance, How best can embassy officials in Mexico City and Washington get their points of view across to key personnel in the executive and legislative branches of the host country without crossing the invisible line of foreign intervention? Similarly, questions about Mexico's oil policies may not even touch on some fundamental matters of U.S.-Mexican relations, for instance, Did Mexico's oil policies deliver what they promised—and if not, what went wrong?

George Grayson's essays and op-ed pieces on Mexico have appeared in publications as unrelated as the *Petroleum Economist* and the *Christian Science Monitor*. His early book, *The Politics of Mexican Oil* (1980) contained valuable field research on the Oil Workers' Union at a time when little in English was available about Mexico's resurgent oil industry. Grayson tracks the general and the oil trade press carefully for news about Mexico, but he shows little interest in economic or quantitative analysis: a statement describing economic conditions might contain values in both dollars and pesos.

Oil and Mexican Foreign Policy is fundamentally a collection of essays, each of which chronicles—mainly since 1981—an oil-related theme. While each chapter has its own time line, there is a loose shifting of chronological focus from chapter 3, which concerns the anticorruption efforts of the early de la Madrid administration in 1983–84, to chapter 8, which speculates on issues and likely policies of President-elect Carlos Salinas.

Chapter 4 argues that Mexico's presence in Washington has been inadequate, owing, in part, to a sleepy embassy. Chapter 5 gives a blow-by-blow account of how Mexico finally got trapped in OPEC's spiderweb only to discover in 1985–86 that the cartel's market credibility and internal discipline were things of the past. Chapter 6 chronicles the story of the San José Accord, which was Mexico's attempt to show the world that a larger vision of world oil trade—beyond that of the venal spot market—could be applied in a multinational context (Caribbean Basin countries, exc. Cuba). Chapter 7 narrates Mexico's liaison with Nicaragua.

Chapters 1 and 2 introduce elements of a theoretical framework, one in which countries are conceived of as having a predominate image of themselves in international affairs. The framework is only occasionally worked into the rest of the book.

An original line graph (Fig. 2, p. 28) shows the weighted price of Mexican oil (dollars per barrel) from 1977 to 1988. Superimposed on the price line are markers showing major moves in Mexican foreign policy. The graph suggests that as oil prices rose, Mexican foreign policy became more aggressive and ambitious. The San José Accord, for example, came into effect shortly before the highest price was reached; and selling oil on contract to the U.S. Strategic Petroleum Reserve—the real scabbing on OPEC (in my view)—occurred after the oil glut had set in.

The chapter on U.S.-Mexican relations contains fresh material based on interviews. Grayson records how, following the bad U.S. press associated with the 1985 state and local elections, the Office of the President of Mexico (OPM) concluded that it needed a public relations firm to promote Mexico's image. The OPM hired the firm of Deaver and Hannaford for $250 thousand annually. Peter Hannaford soon broke with scandal-ridden Michael K. Deaver and took the Mexican account with him. In Grayson's eyes, the new account manager, Mark E. Moran, of the new firm, Hannaford Company, was active and efficient, on an account now billed at $360 thousand annually, plus expenses. Moran's qualifications included experience on the staff of the Senate Foreign Relations Committee (p. 102). Grayson incorrectly states that one of Moran's ideas was bridge building with the National Association of Hispanic Journalists.

Concerning the Washington scene, Grayson, by omission, does a disservice to the work both of the Mexican ambassador, Jorge Espinosa de los Reyes, who is quoted only once (in contrast the lengthly treatment of Moran) and of the U.S.-Mexico Chamber of Commerce (USMCOC), which has been headed since 1978 by Gerald J. Van Heuven, who has an industry background in Mexico. The USMCOC was a Mexican idea conceived of by former ambassador Hugo B. Margáin. For a time, the USMCOC's doors were kept open by funds from Mexico's Foreign Trade Institute (IMCE). The USMCOC, which subsequently encouraged the opening of self-funding, regional chapters in Los Angeles and elsewhere, sponsors seminars and field trips to Mexico.

An omission of greater significance is the absence of a chapter on the Pacific Basin and Japan. What were the package deals that Mexico negotiated with Japan to get Mexico to build an oil pipeline from the Caribbean-side oilfields to the Salina Cruz terminal on the Pacific Coast?

Grayson is an able chronicler of the day-by-day infighting of international politics. He is not a theorist, nor does he offer any fundamental critique of the conduct of Mexican policy makers in the area of oil policy. In his conclusion he wants to give answers to questions that the book did not address, such as How should the Mexican government size up the complex question about how to conduct itself in Washington? Will the course of U.S.-Mexican relations go smoother if issues as diverse as trade, drugs, debt, oil, and immigration are negotiated as a package or as individual, compartmentalized topics? The answers to such questions do not come out of a narration, however careful, of Mexico's oil policies vis-à-vis OPEC or Nicaragua.

GEORGE BAKER

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Power and Tactics in International Negotiation: How Weak Nations Bargain with Strong Nations. By William Mark Habeeb. Baltimore: Johns Hopkins University Press, 1988. 168p. $25.00.

Much of the recent literature on international negotiations deals with theoretical and methodological approaches to the study of the processes and outcomes of bilateral negotiations and offer practical suggestions to negotiators. Very little attention is devoted to asymmetrical negotiations. Most conceptual, methodological, and theoretical frameworks do not adequately explain why weak international actors have sometimes been able to achieve many of their objectives in their negotiations with great powers.

Habeeb's book develops a power theory for the study of the processes and outcomes of asymmetrical negotiations, applies this theory to three cases of negotiations between weak and great international actors, and offers recommendations to negotiators for maximizing their abilities to achieve their desired goals in the negotiation process. In the opinion of the author, most of the negotiation theories—including game theory, concession-convergence theory, and psychological-behavioral theory—offer a narrow understanding of this multifaceted phenomenon. The classical power theory is also inadequate for its conceptual limitations and its general assumption that in bilateral international negotiations powerful actors always achieve many of their objectives.

Habeeb dismisses the classical conception of power as being too static and develops a three-dimensional power concept consisting of both structural and behavioral powers. Structural power includes the aggregate structural power (which essentially "refers to an actor's resources, capabilities, and position vis-à-vis the external world as a whole" [p. 17]) and the issue-specific structural power (which "is concerned with an actor's capabilities and position vis-à-vis another actor in terms of a specific mutual issue" [p. 19]). Behavioral (or tactical) power is an actor's ability to use these resources to attain its objectives. Based on this conceptual framework, the author develops a theory that presumes that the process and outcome of any asymmetrical negotiation is essentially determined by the dynamic changes in the balance of issue-specific power. Whereas aggregate structural power has a major direct and indirect (through its impact on the tactical power) influence on negotiations, it does not determine the process and outcome of negotiations. The balance of issue-specific power is determined by the three interrelated variables of alternatives, commitment, and control. The availability of alternatives, intensity of commitment due to aspiration rather than need, and the ability to unilaterally achieve the preferred outcome despite the cost increase an actor's issue-specific power. Conversely, lack of alternatives, limited commitment or high commitment due to need, and the inability of the actor unilaterally to achieve its goal diminish the issue-specific power of the actor. Contextual factors such as the domestic and international environments, leadership, and so on are also instrumental in shaping the direction of power imbalances in the negotiations.

According to this framework, the negotiation process consists of each actor's attempt to alter the issue-specific power balance in a more favorable direction. The formula detail approach that examines the negotiation process in three essential phases—the diagnostic phase, the formula phase, and the detail phase—is a useful tool for analyzing the dynamic process of negotiation in view of periodic imbalances in the issue-specific power. Habeeb applies this framework to bilateral negotiations involving weak and strong actors in the cases of the Panama Canal negotiations, U.S.-Spanish bases negotiations, and the Anglo-Iceland cod wars negotiations. He skillfully illustrates how changes in the issue-specific power—rather than the aggregate structural power—at various stages of these negotiations ultimately determined the directions of the processes and the outcomes of negotiations.

Although Habeeb raises valid criticisms of the traditional theories of international relations and the concept of power, his framework is not completely free from conceptual and methodological shortcomings. First, while classifying power into structural and behavioral categories and underscoring the role of issue-specific power in negotiations are useful, they do not provide any new insights into the old methodological problem of measuring the complex phenomenon of power. Second, the framework does not establish rigorous guidelines for investigating the relative importance of the components of power in different phases of negotiation. Third, as evidenced by the case studies, contextual factors might play a determining role in the direction and outcome of negotiations. The framework accounts for these factors but does not clearly establish their relationship to power variables.

Despite these shortcomings, Habeeb's work is a theoretically important step towards developing a rigorous framework for the study of asymmetrical bilateral international negotiations. However, as he astutely notes, much needs to be done in the form of establishing further empirical support for the theoretical findings of this model and overcoming its methodological shortcomings.

MEHRAN TAMADONFAR

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Statecraft, Domestic Politics, and Foreign Policy Making: The El Chamizal Dispute. By Alan C. Lambborn and Stephen P. Mumme. Boulder: Westview, 1988. 211p. $18.00 paper.

The authors of this book intend to develop a theoretical framework to allow for the integration of statecraft (interstate) and domestic factors that influence foreign policy choice and the ability to implement choices effectively. They analyze the case of El Chamizal, a long-standing dispute between the United States and Mexico over land transferred from Mexico to the United States by a change in the river channel of the Rio Grande in the nineteenth-century. The goal is to create a modern theory of foreign policy choice that builds on the classic statecraft tradition, on the domestic sources discussed by Graham Allison, and on the strategic sources analyzed by Bruce Bueno de Mesquita.

The first part of the book develops the analytical framework and ranges from Machiavelli to Morgenthau to E. H. Carr. The basic point appears to be that when a hegemonic state such as the United States is bordered by a smaller and weaker state, questions and values other than simple power may enter into the relationship. The weaker state—in this case Mexico—may be marginal to the hegemon and thus permit some degree of altruistic behavior, which is ultimately what happened in the Chamizal dispute. For much of the history of the dispute, however, the United States appeared less than interested in any sort of settlement, and it was not until the United States wanted to forge a new hemispheric order after World War II that realistic negotiations became possible. At the conclusion of this section the authors adopt a key faction approach to effective political action. Two types of domestic coalition building are basic to policy choice: (1) coalition building among the key factions among foreign policy decision-making coalitions and (2) coalition building among the key socioeconomic groups found in the political structure of the larger society.

The remainder of the theoretical chapter is devoted to a good critique of Allison's three models and Bueno de Mesquita's expected utility theory of strategic choice. The goal is to present a "convincing demonstration of the logical necessity" of integrating strategic factors of statecraft with domestic coalition building for the internal validity of research on both statecraft and foreign policy analysis.

The second part of the book deals with the Chamizal dispute. Conventional historiography on the dispute treats it from the statecraft perspective with a unitary actor, the nation-state, acting in terms of rational choice in response to the international environment. The United States initially rejected the 1911 arbitration agreement and continued to oppose any negotiations based on that agreement for the next 50 years. There was short-lived effort under President Hoover to resolve the issue, but it got nowhere. Then President Kennedy finally was able to resolve the dispute in 1963 because of his concern for the threat posed by Castro to hemispheric peace, and he basically conceded to the Mexican position in order to gain support for overall U.S. interests in the inter-American system.

Few would dispute this conclusion. However, using recently declassified materials, the authors show that far from its coming out of a clear blue sky, secret negotiations had been going on during both the Truman and Eisenhower administrations. The book traces these negotiations in some detail and shows that what blocked a statecraft solution to the Chamizal was the failure to build an effective coalition domestically in support of a solution. Indeed, the opposite happened. The International Boundary and Water Commission was opposed to concessions to Mexico and effectively utilized the Texas delegation in Congress to block or delay executive initiatives. Neither Truman nor Eisenhower could break through the coalition against a Chamizal settlement.

The situation changed with Kennedy. First, the Texas delegation in Congress was not in the strategic position it had been in the past. And as vice-president, Lyndon Johnson used his abilities to buy off opposition to the settlement in Texas. Finally, Kennedy went public and no one wanted to challenge presidential authority.

Students of U.S.-Mexico relations should read this book for an understanding of the importance of domestic groups in formulating policy towards Mexico. This is especially important for borderlands scholars dealing with a host of issues such as dope trafficking, immigration, pollution, trade, and so on. One drawback to the analysis of this book—readily acknowledged by the authors—is that a better analysis would come from understanding the same coalition-building process within Mexico, but the information is simply not available. Perhaps it would have helped the reader if the theory had been spelled out in more detail, since there seems to be some confusion about the limits of the analysis. Nevertheless, it is a valuable contribution to the literature on foreign policy decision making and should prove especially worthy for graduate students to view theory applied in a concrete case.

C. Richard Bath

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External Debt, Savings, and Growth in Latin America. Edited by Ana Maria Martirena-Mantel. Washington: International Monetary Fund, 1987. 207p. $12.00 paper.

Crises in the Caribbean Basin. Edited by Richard T. Tardanico. Newberry Park, CA: Sage, 1987. 264p. $29.95.

The Martirena-Mantel text presents an extremely detailed but nontheoretical analysis of a limited set of problems, whereas Tardanico's volume tries to apply a consistent theoretical perspective to a variety of separate cases that are included solely on the basis of their geographic location. Neither volume has an index, nor are there integrated bibliographies.

The subject matter of the Martirena-Mantel volume is a familiar one: analysis of the causes of debt crisis in Latin America, the relationship between debt and economic growth problems, and suggestions for solutions. There are differences among the papers, but overall the analysis is extremely technical. The authors focus almost exclusively on purely economic questions, generally provide detailed sets of data, and treat policy issues in a fairly technocratic style. The volume seems aimed at providing good technical data for the International Monetary Fund (IMF) and other policy makers, and the several separate sets of references would combine to form a useful bibliography only for the narrow specialist.

Martirena-Mantel's introductory chapter, for example, is essentially atheoretical, and sets the tone for the entire volume. Her essay summarizes each of the book's papers in great detail—a useful assist for most readers. But on policy issues, she neglects the larger questions, for instance there is no acknowledgment of the question of the impact that a more consistent set of macroeconomic policies on the part of the lending nations would have on other dimensions of social life. The social and political impact of IMF actions, for example, is not mentioned. There is no explicit discussion of political economic theory writ large—in contrast to the Tardanico volume. For economists and others who specialize in the debt crisis and growth and savings issues, this volume is a valuable source. For other political scientists, the absence of macropolitical theory makes this volume less useful. There is probably more here about the debt crisis and possible policy responses to it than most nonspecialists will want to know.

Tardanico notes that political and economic crises in the Caribbean region have been regular features in the U.S. press and that policy makers have been preoccupied with the region for over a decade due to revolutions, insurgencies, and civil strife, all situations that have been perceived as threats to North American hegemony in the region. This volume seeks to present a theoretically based analysis of these crises and of U.S. policy responses. Interestingly, there is no discussion of the sustained patterns of gross violations of human rights in some nations in the region, which tells us more about the perceptions of North American policy responses than it does about the editorial position of this volume, which is generally critical of U.S. policy.

The strength of the Tardanico volume is its theoretical approach, a structural argument that uses a long-range perspective on policy problems, focusing on "structures" that provide "windows of opportunity" for policy makers and at the same time limit choices. There is a strong sense of historical real time, the notion that yesterday's policy responses become the structure of today's problems and policy choices and that today's policy options need to be analyzed not merely as technocratic responses but as the architects of tomorrow's policy agenda. More specifically, the "theory" is Wallerstein's world system approach to development, albeit loosely and inconsistently applied in the various chapters. Wallerstein's political economy approach assumes that political processes reflect economic institutions and that the economy is essentially a "world system." The approach, as Tardanico describes it in his introduction, certainly acknowledges the limits imposed on small national economies by the capitalist economy of the Western world. But unlike orthodox dependency theorists, Tardanico insists that these nations have room to maneuver—opportunities that can be seized. To understand politics therefore means to cast the analytic net as widely as possible in order to ensure a thorough analysis of the structures that impinge on policy choices and political action. Jeffery Paige's study, "Coffee and Politics in Central America," serves as an illustration. He uses excellent historical data to show that past policies regarding economic structures (like land tenure) have produced current revolutionary situations and the consequent need for counterrevolutionary states and their respective styles.

Once past this unitary theoretical perspective, however, there is little consistency in this volume. First of all, as the editor acknowledges, the approach itself is inconsistently applied: contributors adhere to a "structural" style, but not all seem to be consciously applying Wallerstein's variant. Moreover, topics vary greatly. Scholars interested, in particular, in Central America and Guatemala will find an excellent chapter by Ralph Lee Woodward. Scholars of ethnicity will find a valuable contribution by Sidney Mintz. Students of the Caribbean islands will appreciate David Bray's excellent cross-national comparison of recent development strategies in Jamaica and the Dominican Republic. Other chapters focus on the International Monetary Fund (IMF) the informal sector in Colombia, the Sandinista foreign policy of nonalignment, and so forth. Few if any scholars will be interested in such a breadth of topics, though probably most should pay attention to this volume's contribution to their particular field of interest. With few exceptions, each author's set of references are good, and some are excellent. But only the scholar particularly interested in studying how Wallerstein's world system approach can be used in various areas will appreciate the entire volume.

If there is any significant crossover factor in these two volumes, it lies in the various references to the role of the IMF. Reading both volumes with that focus in mind illustrates the contrast between them: Martirena-Mantel's contributors are suggesting what the IMF (and other policy makers) might do in the face of current conditions, whereas Tardanico's contributors who study the IMF are seeking to explain why that agency's actions have not succeeded in the past as a way of suggesting that the absence of structural analysis will prevent it from succeeding in the future, too. In short, these two volumes are very different in focus and in style. Tardanico's text advertises a crisis style but actually provides solid, long-range, theoretical analyses throughout. The Martirena-Mantel volume appears (and is) more technocratic and nontheoretical in style but in fact focuses more closely on a recognized "crisis." Ideally, an edited volume should have both a consistent theoretical perspective and in-depth analysis of a topically narrow range of issues, but it is rare to find the combination.

Neither of these two works meets this standard, though both have something to offer.

ROBERT H. TRUDEAU

Providence College

Transportation and World Development. By Wilfred Owen. Baltimore: Johns Hopkins University Press, 1987. 157p. $22.50.

Wilfred Owen, the former director of transportation programs at the Brookings Institution, has managed to deal with the enormous topic of transportation and world development in a brief volume of concise prose. It is hard to imagine someone who would not benefit from reading it.

Owen begins by noting that another billion people will inhabit the earth by the year 2000. These people—and economic growth generally—will create enormous demands for food, housing, materials, waste disposal, personal travel, and so forth in the context of rapid urbanization. All of these developments involve transportation. Moreover, 90% of the population growth will occur in poor countries that lack transportation facilities to begin with and will be unable to afford the additional facilities they will urgently require in the future. The current immobility of some two billion people locks them into poverty and hinders prospects for world development.

After impressing and depressing the reader with an initial barrage of statistics, Owen proceeds to discuss mobility in relation to world food supplies, industrial development, patterns of urbanization, and individual opportunity. His solutions begin with transportation improvements such as unit trains, pipelines, integrated railroad systems, containerization. Second, he suggests limitations on the demand for transport. Rational land use planning for cities, with reference to the positive experience of Singapore, is a major suggestion in this regard. Third, noting that transport systems are always in financial trouble, Owen discusses the thorny political question of resources. He likes the U.S. model of user taxes, linked to deregulation and mixed public-private operating companies. Finally, given the inadequacy of current development assistance, Owen proposes a global tax collected on energy, travel, or trade to be dedicated to a world transportation fund. An inventory of transport conditions and projections of future needs would be provided by an international transportation center linked to the United Nations.

This is a book about numbers, relationships, and contrasts. It is totally convincing in showing the increasing interrelatedness of the world economy and society. Economic links have become incredibly complex; tourism may soon become the world's largest industry. (One wonders, however, why 29 million people per year are said to visit Spain on p. 88, but 25.6 million on p. 90). Some striking contrasts stick in the mind: If everyone in the United States decided to travel by car at the same time, no one would have to ride in the back seat. But in India, 740 people would have to share a car (p. 11). The statistics relating mobility and development are less convincing, particularly since "development" is never defined (see pp. 8–11). Surprisingly, Owen does not mention the recent World Bank emphasis on pick-and-shovel methods of construction and maintenance of penetration roads in Third World countries. (see Basil Coukis's *Labor-based Construction Programs* [1983]).

The planning emphasis of *Transportation and World Development* is a welcome change from the myopic market worship of the Reagan years, but in one respect it is quite inadequate. The book does not seriously consider the ecological implications of its proposals. Like children's transformer toys, "transportation" problems quickly metamorphose from one form into another. Owen may be right, for example, to argue that "motorization of rural road transport is essential to meeting world food requirements and needs to be accelerated in Third World countries" (p. 130). But the implications of large-scale growth of the automobile-industrial complex and the concomitant road building include depletion of the world oil supply—and thus increased costs for agriculture, higher levels of carbon monoxide leading to a "greenhouse" effect, erosion of soils, and deforestation. The limits-of-growth arguments need to be taken seriously. [See Lester R. Brown and colleagues, *State of the World, 1987.*

RICHARD E. HARTWIG

Valdosta State College

Managing the Frozen South: The Creation and Evolution of the Antarctic Treaty System. By M. J. Peterson. Berkeley: University of California Press, 1988. 283p. $35.00.

Antarctic Mineral Exploitation: The Emerging Legal Framework. By Francisco Orrego Vicuña. Cambridge: Cambridge University Press, 1988. 615p. $89.50.

Recent years have seen a significant increase in the number of scholarly and popular books dealing with Antarctica, an increase that is also reflected in magazine articles, television programs, and other forums, such as the General Assembly of the United Nations. This interest shows a growing appreciation that Antarctica and the surrounding waters and islands are a special region with the potential for important international cooperation or conflict. The cooperative aspects stem from the unique Antarctic Treaty System, which provides for international scientific and logistical collaboration, while the conflictive aspects can be traced to competition for potential resources and sovereignty disputes, as well as the fact that the brief but dramatic Anglo-Argentine South Atlantic War of 1982 almost reached Antarctic waters. Growing numbers of Antarctic tourists and new concerns for ecological issues in the region have further heightened interest in Antarctica, while potential resource exploitation and a possible review of the treaty in 1991 put considerable pressure on the existing regime.

The two books reviewed here reflect two currents in the contemporary literature on Antarctica. The first examines the Antarctic Treaty System from the general perspective of international politics as a unique regime with applicability to other regimes, while the second takes a more narrow juridical approach focusing on the specific problem of mineral resource management.

M. J. Peterson's book applies international relations theory dealing with general considerations of regime creation, establishment, maintenance, amendment, and replacement to the Antarctic Treaty System. Her principal variables for analysis are the salience of issues, the definition of national interests, and the forming of coalitions. Using these considerations and variables, she traces the history of the Antarctic Treaty System from its birth (including a false start in 1949), through the drafting of the 1959 Washington Treaty, the subsequent operation of the system, and the present challenge by a number of actors that would seek to replace or fundamentally change the system. As Peterson notes, these actors include nations not party to the Antarctic Treaty System, a Third World movement calling for Antarctica to be "the common heritage of all mankind," resource developers, and ecological and conservation movements of various types that seek to preserve Antarctica as a giant world park.

The main strengths of Peterson's book are the theoretical framework and the lessons she draws from the Antarctic Treaty System for other international regimes. This gives her work an appeal beyond that of the specialist interested in Antarctic affairs. Her systematic and thorough treatment of the regime variables over time are commendable. One might question her argument that domestic political factors seem to have little impact on national policies toward Antarctica. While this seems to be true for nations with little "Antarctic consciousness," such as the United States, it would not seem to hold for the austral nations, especially Argentina and Chile. Even in the United States there is a growing lobby of Antarctic enthusiasts and conservation coalitions, which has exerted pressure on the bodies that formulate U.S. Antarctic policies. Peterson's basic conclusion that the salience of Antarctic affairs is likely to return to its normal low place for a long period to come seems unduly optimistic in light of this interest, as well as of the increasing significance of resource issues.

Peterson's book is well organized, with helpful chapter summaries and clear organization. Her bibliography is extensive, although the inclusion of more non-English source materials (especially those from Latin America) would have led to greater balance.

The work by Francisco Orrego Vicuña more than fills the gap, with a heavy dose of non-English language sources, but is a disappointing book because of its highly specialized juridical approach. Its value is limited by the absence of any theoretical framework (other than international law), as well as its expense, length, and the 200–350 or more footnotes per chapter that clutter the arguments. A further limitation is that the cutoff date for his research appears to be mid-1986. Thus, the initialing of an Antarctic Mineral Convention in June 1988 means that much of his work has now been overtaken by events only a few months after publication. As the author notes in his acknowledgment, the book developed from his Ph.D. dissertation at the London School of Economics and Politics, Orrego Vicuña was Chilean ambassador to the United Kingdom and director of the Institute of International Affairs of the University of Chile. He has written extensively on Antarctic matters, generally from the Chilean perspective. One wonders why the Cambridge University Studies in Polar Research series published this book at this time. It would have been wiser to wait a year or two until the final shape of the Antarctic Mineral Regime became clearer and to hold to their stated criteria of publishing relatively short and fully illustrated works.

The strength of Orrego Vicuña's work is the wealth of legal detail he provides, as well as some interesting analogies he draws between the Antarctic Treaty System and the regime of the Law of the Sea. His extensive bibliography is valuable, although the number of subdivisions makes it difficult to locate material. Unfortunately, Orrego Vicuña's highly detailed juridical treatment leaves out much of the stuff of politics and international relations, including the geopolitical analysis of power politics in Antarctica that explains much of the Latin American approach to that continent and the islands between it and South America. His focus would have been more valuable had it included more materials on Third World and conservationist approaches to Antarctic mineral exploitation.

JACK CHILD

American University

Emerging Doctrines and Technologies: Implications for Global and Regional Political-Military Balances. Edited by Robert L. Pfatlzgraff, Jr., Uri Ra'anan, Richard H. Shultz, and Igor Lukes. Lexington, MA: Lexington Books, 1988. 326p. $42.00.

This is a collection of conference papers with a fairly straightforward unifying theme: the United States and its allies are facing a growing and basically unchecked threat in the strategic doctrines and military technologies developed by the Soviet Union. The implicit (and often explicit) assumptions are that doctrine is very important for the application of military weaponry and that the shifts occuring in military technology are indeed major enough to require continual and substantial amendments of doctrine.

Several of the chapters basically amount to historical case studies with a few conclusions of contemporary relevance tacked on at the end, while other chapters are well-stated presentations of an argument that the United States needs to do more to defend itself and its friends.

Missing from the collection is any serious presentation of opposing viewpoints. For example, the possibility is not considered that the overwhelming destructive power of nuclear weapons may not make Soviet-U.S. variations in doctrine—or any shifts in either side's doctrine over time—quite unimportant. The chapter by Ra'anan and Shultz and the chapters by Lukes and Deane and Vigor and Ramsey, all make the case that Soviet military doctrine is something that has to be studied closely, because it may equip the Soviets to take the offensive against the West or at least will *dispose* the Soviets in this direction.

Also glaringly missing—for a book published in 1988—is any reference to theories according to which Soviet doctrine is indeed important but Mikhail Gorbachev may substantially change Soviet outlooks on foreign policy and military policy. All but one of the passing reference to the Soviet leader in the book are tucked into the one chapter by Norman Friedman on maritime doctrines and technologies. One does not have to be taken in by the promises of a new and reformed Soviet Union to address at least the possibilities here and to discuss whether expressed Soviet military doctrines may not—contrary to the view expressed in Peter Vigor's chapter—at last begin to recognize and accommodate West European fears of the Soviet tank force offensive potential.

The book thus offers the reader some articulated statements of the threats posed by Soviet preparations for land and sea warfare, chemical and biological warfare, and strategic nuclear warfare. It also offers a ringing endorsement, in the chapters by Alcala and Pendley, of "airland battle" and the "maritime strategy," doctrines developed by the U.S. Army and Navy, respectively, for counteracting a Soviet conventional invasion of Western Europe. But these chapters are too short and too direct to get into all the many doubts that have been voiced on the workability of these preparations for defending NATO by nonnuclear means. Most defense analysts may thus continue to see the most important debate as being not about Soviet doctrine or intentions but rather about the practicability of extended nuclear deterrence versus a conventional defense.

For a reader wanting to confront very broadly the kinds of estimates of the situation that the Reagan administration found persuasive three or four years ago and that Defense Secretary Caspar Weinberger continued to endorse to the end of his term of service, this will be a handy collection. Anyone looking for a more complicated or timely analysis may be more disappointed, however.

GEORGE H. QUESTER

University of Maryland

Inside Terrorist Organizations. Edited by David C. Rapoport. New York: Columbia University Press, 1988. 259p. $35.00 cloth, $14.50 paper.

Democracies against Terror: The Western Response to State-supported Terrorism. By Geoffrey M. Levitt. New York: Praeger, 1988. 142p. $32.95 cloth, $9.95 paper.

In his introduction to *Inside Terrorist Organizations,* editor David C. Rapoport observes that much of what purports to be a literature on terrorism is actually a literature on counterterrorism. This collection of essays is a welcome addition to the growing body of works that take terrorist organizations seriously enough to study them as authentic political subjects rather than seeking merely to denounce them, explain them away, or prescribe methods of combating them. Serious scholars of political violence will do well to examine these contributions carefully.

Their focus, collectively, is on the internal dynamics of terrorist organizations, in particular, on patterns of organizational behavior and sources of ideology. This division of topics corresponds roughly to the two basic approaches to terrorist behavior discussed by Martha Crenshaw in the collection's lead essay: the "organizational" and the "instrumental" (p. 13). According to Crenshaw, instrumentalists see terrorism as primarily intentional, emphasizing its ideological character and strategic rationality, while organization theorists see it as the product of a more complex mix of personal intentions and organizational imperatives, including role maintenance, factional competition, costs of entry and exit, and the like.

Clearly, Crenshaw prefers the latter perspective to the former; and there is no doubt much is to be gained by applying the insights of organization theory to the activities of terrorist groups. On the other hand, there is more to life—and to terrorism—than this instrumental/organizational dichotomy can comprehend. It is the nexus between organizational and other factors, between terrorist groups and their societies, that most needs explaining. Only a few of the case studies presented here shed light on this problem. Most provide raw material for future theory rather than evidence supporting any existing general theoretical model. But this is also the book's strength. When thoughtful and detailed case studies allow the data demonstrating patterns of organizational or ideological development to speak for themselves, we acquire new understandings of organizations whose inner workings were previously obscure.

Thus, Ronald Crelinsten demonstrates that the Quebec Liberation Front, often thought to be an efficient, centralized organization, was in reality a "two-headed monster," whose internal differences had much to do with determining the outcome of the Cross and Laporte kidnappings (p. 78). David Rapoport uses terrorist memoirs to show how the "internationalization" of terrorist groups has tended to weaken and divide them (p. 52). Gordon H. McCormick considers the complex internal factors that have contributed to the unusual success of the Peruvian Sendero Luminoso, and Ehud Sprinzak offers an incisive short history of the evolution of Gush Emunim's ideology and tactics. The best essays here are the least patronizing. David Schiller's account of internecine conflict within the PLO is marred by an overstated emphasis on Palestinian "tribalism" (p. 92), while Bonnie Cordes's narrow focus on the "auto-propaganda" (p. 151) aspects of West European terrorist communiqués adds little to our understanding of the anarchocommunist groups.

Interestingly—and, perhaps, for political scientists, embarrassingly—the outstanding theoretical contributions to this volume are made by a literary critic and a professor of ethics and religion. In "Cultural Narrative and the Motivation of the Terrorist," Khachig Tololyan, coeditor of the journal *Pynchon Notes*, argues convincingly that Armenian terrorism (and, by implication, the terrorism of other nationalist groups) can be properly understood only in the light of the community's "projected narratives"—popular stories that "tell individuals how they would ideally have to live and die in order to contribute properly to their collectivity and its future" (p. 218). Mark Juergensmeyer's essay on Sikh terrorism explores "the logic of religious violence," arguing that while political, economic, and social explanations for religious violence may be correct as far as they go, it is also necessary to understand the thinking of those who believe themselves to be involved in a cosmic struggle that is at a point of crisis (p. 172). While neither essay attempts to connect cultural paradigms with other social factors in a systematic way, both enrich our understanding of the terrorist sensibility.

One hesitates to close a generally appreciative review on a negative note, but I am constrained to add that the technical editing and production of *Inside Terrorist Organizations* is little short of abominable. Its type is ugly and crowded, its margins are nonexistent, the jacket cover is amateurish and silly, and I counted 28 serious typographical errors (e.g., "Maorist" for "Maoist," p. 6) without trying.

Democracies against Terror: The Western Response to State-Supported Terrorism is a short monograph with a documentary appendix by Geoffrey M. Levitt, an attorney-advisor to the State Department Office of the Legal Advisor during the Reagan administration. The book's sweeping title is somewhat misleading, since the study focuses almost exclusively on initiatives taken by the Summit Seven nations (the United States, Canada, the United Kingdom, France, the Federal Republic of Germany, Italy, and Japan) during the 1980s against certain types of terrorism—in particular, airplane terrorism, attacks on internationally protected persons, and abuse of diplomatic immunities. Levitt describes these initiatives in some detail, illustrates their application (or nonapplication) to six terrorist incidents occurring during the 1980s, and concludes by discussing the "essentials of a collective response" (p. 93).

This is a judicious exploration, overall, of the strengths and weaknesses of attempts by Western governments to cooperate publicly in punishing those they consider terrorists and the regimes that give them support or sanctuary. A certain amount of lawyerly folderol intrudes. Thus, while conceding that the *absence* of international consensus on terrorism required the Summit Seven to act alone, Levitt is wont to refer to "international norms" condemning terrorism and the states that support it (e.g., p. 102). Nevertheless, he recognizes that even among the Summit Seven the essentials of an effective collective response—consensus, credibility, clarity, and consistency—have often been lacking and discusses the problem of consistency with particular acuteness. Policy makers should find this book particularly interesting. I would recommend that they read it along with Grant Wardlaw's subtle concluding essay in *Inside Terrorist Organizations*, "Terror As an Instrument of Foreign Policy."

RICHARD E. RUBENSTEIN

George Mason University

The Mobility of Labor and Capital: A Study in International Investment and Labor Flow. By Saskia Sassen. Cambridge: Cambridge University Press, 1988. 224p. $34.50.

Reflecting the advent of immigration as a prominent issue in most advanced industrial societies, in the past decade-and-a-half the study of international migration has evoked unprecedented interest among social scientists and undergone thorough theoretical renewal. Fundamental to the new approach is an understanding of the multifarious advantages capital derives from the use of foreign as against native labor, of how such considerations shape immigration policies, and of the role of "core" economies in triggering the onset of emigration within specific parts of the "periphery." Saskia Sassen, who recapitulates the most significant of these insights in chapter 2 of the present book, herself contributed to the literature by way of a succession of stimulating essays exploring the relationships of international migration to changing features of the international political economy. We are fortunate now to have these essays—hitherto scattered in publications unfamiliar to political scientists—in book form, with a clearer statement of the overall theme and additional empirical illustrations.

The central theme is captured by the title and subtitle. In short, Sassen argues that the internationalization of production sites resulting from investment by advanced capitalist countries in the Third World has largely shaped the direction and characteristics of contemporary labor migrations. The analysis is historically specific. Export-led industrialization emerged as a new development strategy in the 1970s to replace import substitution industrialization because of limited internal markets and the search by multinationals for cheap labor locations. Because this meant, in effect, a shift to relatively high value-added light industries (electronics, textiles, clothing), young women constituted an increasing proportion of the labor force. This, together with the transformation of subsistence farmers into wage-workers, disrupted household production and contributed to male unemployment. Unemployment triggered migration within the region, with the overflow spilling over abroad.

The approach parallels that of A. Portes ("Migration and Underdevelopment," *Politics and Society* [1978], who emphasized penetration by a core country into a part of the periphery as the principal determinant of the onset of a migratory stream. However, Sassen's proposition concerning the unprecedented mobilization of women into the labor force is quite distinctive, as is her emphasis on concomitant and complementary processes on the "demand" side. She argues that the counterpart of decentralization of manufacturing (and also of routine white-collar office jobs, rendered possible by computer-cum-satellite technology) is the centralization of global management and servicing of this far-flung and multifarious market in "global cities" such as New York and Los Angeles, which generate a continuing demand for immigrant labor. Her exploration of the changing social structure of these cities (chapter 5) is the most innovative feature of the book, whose relevance goes well beyond the field of migration research. Although she does not deal with the political consequences of these changes, her findings challenge political scientists to reflect on the problems of governance of socially polarized global cities within a constitutional system that renders municipal government peculiarly parochial.

Regrettably, the presentation of empirical materials in support of the theoretical analysis is flawed. Sassen seeks to demonstrate that the characteristics of the "new immigration" of the 1970s confirm her hypotheses concerning the processes at work in the sending country. But the evidence she is able to muster is mixed, and her reading of it confused. She elaborates her case by pointing out that most poor countries do not reach the 20 thousand annual limit of admissions to the United States allowable under current immigration law (p. 9). But there are over 150 countries in the world, and if each sent as many as 20 thousand people, the total would amount to three million—approximately ten times the annual quota limit. The 20 thousand figure is not an entitlement but an upper limit. Many countries do not reach it because they do not generate a sufficient number of qualified applicants for U.S. immigration visas under either the family reunion or high-skill priorities. And their potential emigrants may also have alternative destinations (e.g., Algeria). Table 3.9 (p. 71) does show that U.S.-connected countries such as the Philippines and Korea contributed heavily to the new immigration. But, surprisingly, it makes no reference to nonconnected India, whose nationals, as Table 3.12 shows, now constitute the second largest Asian group in New York City. In any case, most Asians did not come as labor in the conventional sense. As Sassen himself indicates (p. 75), among the 1970 entrants almost 30% reported professional or technical occupations (approximately twice the level of resident foreign-born). And this level still prevailed in 1976. Her evidence concerning the female character of immigration is also questionable. Table 3.5 displays top immigration flows by source country and sex. Sassen states, "A rather clear pattern emerges, . . . The new immigration countries . . . mostly have more females in their flows. The traditional immigration countries . . . have mostly males" (p. 65). However, we are not given the traditional countries at all. And the range among the major sources of new immigration shown in the table is from a high of 60.8% female (Korea) to 48.1% (India), with a median of 52.5%. This is hardly a pattern of female dominance; and one might equally interpret the data to indicate "family," rather than "labor," immigration.

However, paradoxically, these weaknesses do not significantly undermine her theoretical case because their is no reason to expect that the composition of immigration into the United States would reflect precisely the forces at work in countries of origin. Transformations such as the ones Sassen points to foster a disposition to take advantage of *opportunities* to emigrate and preferences for particular destinations; but ultimately, a major determinant of who enters the country of destination is the immigration policy of that country (in the broad sense, including negligent enforcement of borders), which operates a selection from the potential pool. Sassen repeatedly suggests that the reform of U.S. immigration in 1965 was designed to serve its new economic requirements (e.g., pp. 114, 118). But this is the least persuasive part of an otherwise sound analysis, because the law was shaped by a complex array of forces, some of which had little to do with the economy. And as she herself points out, the fact that Asians replaced Europeans as the leading groups from outside the Western Hemisphere was a largely unanticipated outcome.

ARISTIDE R. ZOLBERG

New School for Social Research

Deadly Paradigms: The Failure of U.S. Counterinsurgency Policy. By D. Michael Shafer. Princeton: Princeton University Press, 1988. 331p. $34.50.

Michael Shafer makes some powerful arguments in this book, and he does not mince words in making them. His principal contention is that U.S. counterinsurgency strategy has been a double failure. In cases like Vietnam, it brought abject defeat. In cases like Greece in the late 1940s and the Philippines in the early 1950s, where U.S. allies did succeed in defeating insurgencies, Shafer argues that these victories were *in spite of*, not because of, U.S. assistance and guidance.

The subject matter itself clearly is of great interest, both because of its historical significance and its contemporary policy relevance. Shafer also structures his study so as to address the debate within international relations theory over alternative explanations of "the sources of American foreign policy" (p. 3). He argues for the greater relative utility of a cognitive content explanation over realist, domestic structural (president-centered and bureaucratic politics) and political cultural (American exceptionalism) paradigms. Both the substance of policy and its consequences are best explained, he contends, "in terms of the systematic distorting impact of a coherent set of widely shared and unchallenged assumptions concerning the sources, nature, direction and potential consequences of political change in the Third World" (p. 7).

In taking this theoretical approach Shafer's book stands with the work of a number of other scholars (e.g., Robert Jervis, Alexander George, Ned Lebow, John Odell, and Judith Goldstein) who have been developing and applying cognitive-based explanations for international relations generally and U.S. foreign policy in particular. This trend towards cognitive-based explanations within the discipline points to a basic dissatisfaction with the more traditional institutional and interest-based paradigms. Shafer manages to express this dissatisfaction and display the virtues of a cognitive approach, yet he does so while still allowing for a degree of complementarity among the distinct paradigms (p. 277). This openness to integration and synthesis is a far more productive orientation than the simplistic either-or epistemology that so often posits differing explanations as strictly mutually exclusive.

Shafer also shows how the methodological obstacles inherent in making a factor as nonconcrete as perceptions-beliefs-ideas to be the principal independent variable can be effectively managed. He dedicates an entire section of the book (chapters 3–5) to an analysis of the social science etiology and doctrinal bases of U.S. counterinsurgency strategy. Both here and elsewhere his archival work is extensive and brings into the literature a great deal of new material. He also is very explicit and self-conscious in specifying his constructs and in applying his comparative case methodology.

Carrying this through in a systematic fashion, however, can be problematic. On the one hand, it becomes cumbersome for the reader to run through each alternative model repeatedly as it fits or doesn't fit different comparisons being made. On the other hand, in the Philippines case, Shafer's presentation of the domestic structural explanation is somewhat cursory (pp. 217-18). Nor, in this case, does he adequately differentiate the realist from the cognitive content explanations.

In terms of the central arguments he advances, he is quite convincing as far as Vietnam is concerned. I am less persuaded, however, by his contention that U.S. policy deserves no credit for the defeats of the Greek and Filipino insurgencies. It is one thing to say that the United States should not be given all of the credit, as some are wont to do. But the claim that the U.S. role was "irrelevant" (p. 238) also strikes me as unduly imbalanced, both empirically and in terms of the analytic and methodological difficulties in attributing (and nonattributing) causality in such situations. A more differentiated analysis getting at the contingencies, conditionalities and probabilities of when, how, and why counterinsurgency works and doesn't work would be more helpful, both in terms of theory development and policy prescription.

Nonetheless this book stands as a solid contribution to the historical record, as an impressive contribution to the scholarly debate on the sources of U.S. foreign policy, and as a study with important implications for policy issues that are likely to stay high on the agenda for the foreseeable future.

BRUCE W. JENTLESON

Cornell University

Soviet-American Competition in the Middle East. Edited by Steven L. Spiegel, Mark A. Heller, and Jacob Goldberg. Lexington, MA: Lexington Books, 1988. 392p. $42.00.

This book is a fine collection of essays on the evolution of U.S. and Soviet policies towards the Middle East. Although the book may have been intended to cover the whole gamut of Middle Eastern regional issues, its focus is primarily on United States and Soviet approaches to Israel and to the confrontation. The strength of this book, however, lies not in what the authors say but in the type of questions they raise about some of the most important issues of our times, namely, the role of the superpowers in times of peace as well as in times of war.

The editors state at the outset that the book's main theme is the limits on superpower influence in the Middle East. Nowhere is this more evident than in the chapters dealing with arms control and technology transfer. The authors successfully illuminate some of the dilemmas faced by the superpowers in controlling the arms buildup in the Middle East and in containing the unintended consequences of technology transfer to the region. One such argument is advanced by Shai Feldman who discusses the domestic constraints that superpowers must face in implementing an effective nonproliferation policy in the nuclear field.

The chapters on arms control allude to the significant conceptual finding that most confrontation states—Israel included—occasionally find themselves for reasons beyond their control having to limit their arms and technology import due perhaps to their limited and fluctuating resources (i.e., during periods of economic and social turbulence). This situation forces the superpowers to "absorb the cost of transferring new technology" (p. 89).

The rest of the book covers an array of topics ranging from military and economic interests to regional and international implications of superpower rivalry. Three case studies by Mark Heller, Sam Lewis, and Barry Rubin are especially interesting.

The book suffers from the lack of a serious theoretical treatment. It does not tie the various chapters to any specific theoretical framework and as a result is disjointed. It does not focus on the broad strategic issues facing both the U.S. and USSR.

None of the authors bother to tell us why the notion of superpower competition is so significant. I suspect that this probably has to do with the problem of political instability on a regional as well as an international level. Whereas it is conceivable to conclude—as the book does—that superpower competition has been a destabilizing factor in U.S.-Soviet relations, I can envision several scenarios where this need not be so. In such cases as terrorism against either side (especially in cases where one superpower could conceivably share intelligence information about an imminent attack by some Middle Eastern groups against the other superpower); Islamic extremism, which has been anti-U.S. as much as, if not more than, anti-Soviet; and the U.S. and USSR's acting to diffuse tensions between their client states (Israel and Syria)—in such cases cooperation, not competition, is more salient.

Nor do the authors discuss the implications or consequences of superpower competition. A comparative analysis of other competitive situations—such as, for example, the type that gripped Europe around the turn of the century, where competition for resources (colonies) abroad finally led to the outbreak of World War One—would have been extremely useful.

Moreover, the authors allude to the limitations of the superpowers without really penetrating to the level of analysis needed to illuminate the dynamics of these limitations. For example, the contributors dismiss (not intentionally, I am sure) the importance of indigenous political cultures, which often limit the ability of either superpower to "control, shape, or even understand political development in deeply foreign countries" (e.g., Afghanistan for the USSR and the Persian Gulf for the United States). The conceptual significance of this argument lies of course in the fact that the authors could have better prepared the stage for a more serious look not only at U.S.-Soviet competition but more significantly at the glaring incompatibilities of U.S.-Middle Eastern and Soviet-Middle Eastern interests. These are the same incompatibilities that often expose the vulnerabilities of the two giant superpowers, allow the other regional players to manipulate them, and most importantly permit the regional actors to question U.S. and Soviet credibility and security guarantees continuously.

What appears to be clear from the various arguments of this book is that neither superpower has stopped seeking unilateral advantage by exploiting the other side's weaknesses. What is missing from such an argument is a discussion about the relationship between domestic policy and political change that contributes to such behavior. Domestic and social determinants in patron states (i.e., the United States and USSR) are no less significant to understanding the dynamics of superpower interaction than are the means (both economic and military) to meet any potential threat by either side in a state of perpetual competition.

KAMAL A. BEYOGHLOW

U.S. Dept. of State

Erratum

Beyond American Hegemony: The Future of the Western Alliance. By David P. Calleo. Reviewed by Margaret P. Karns (March 1989, pp. 339-41). On page 339 the review heading should also include "*U.S. Foreign Policy and European Security.* By Arthur Cyr. New York: St. Martin's, 1987. 156p. $32.50."

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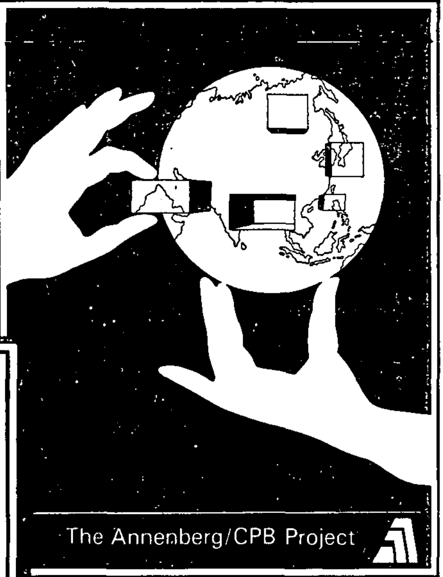

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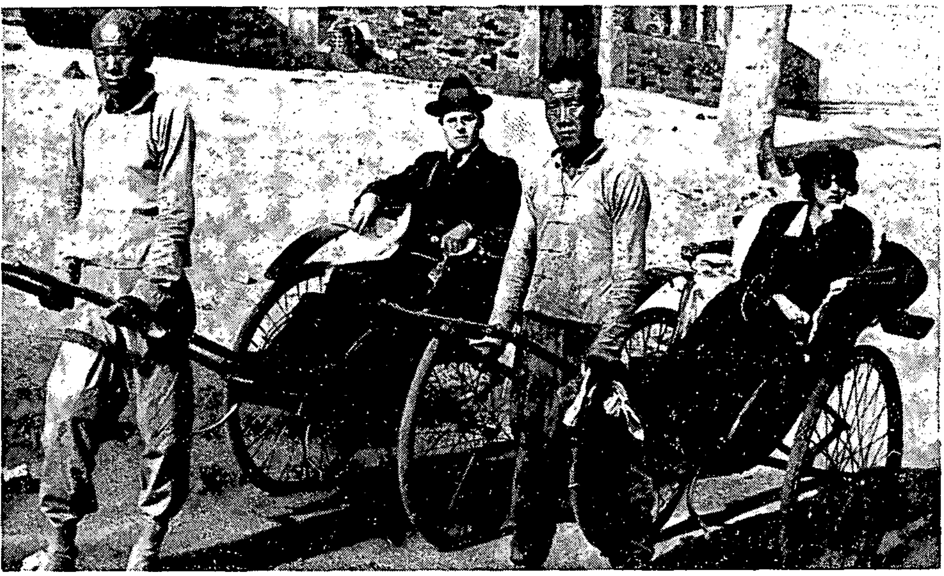

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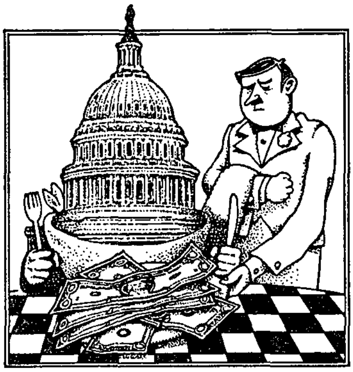

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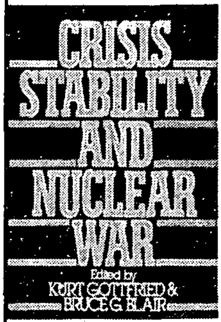
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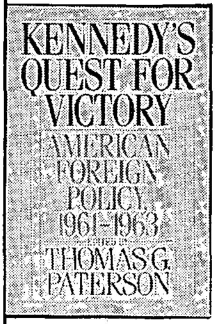
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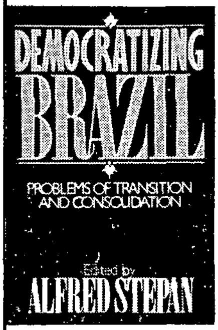
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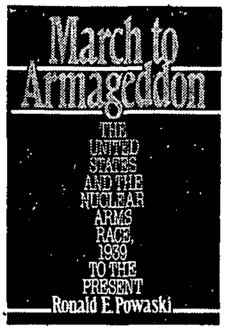
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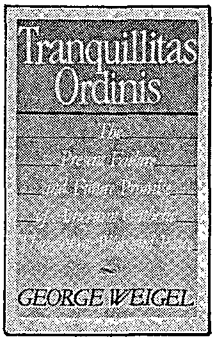
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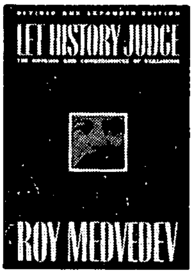
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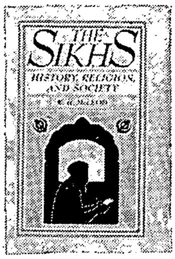

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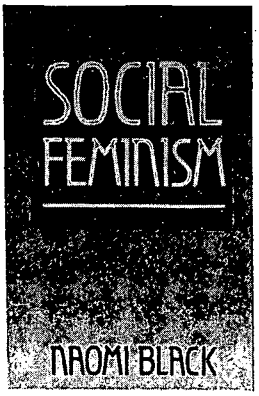

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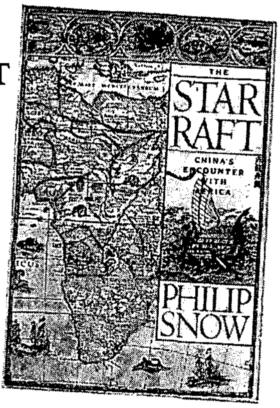

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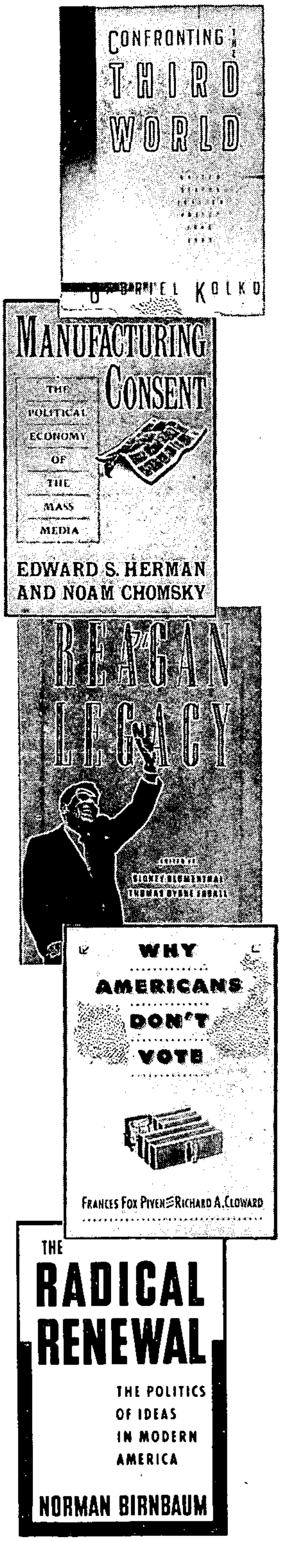
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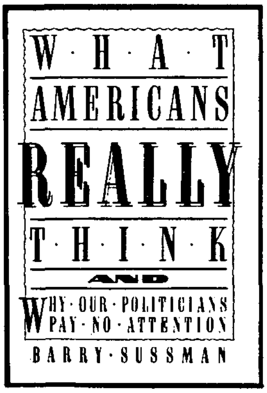

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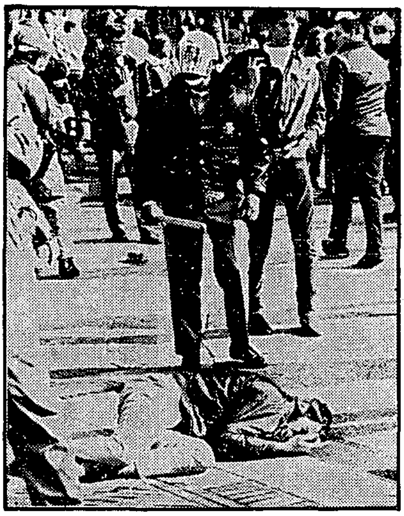

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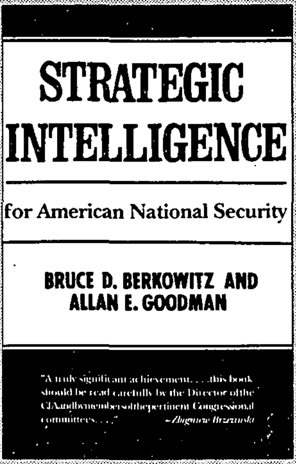
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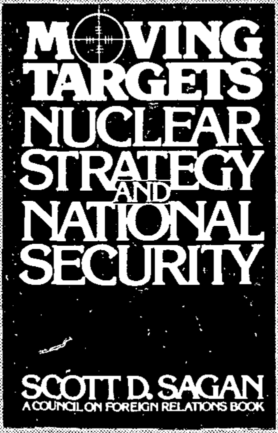
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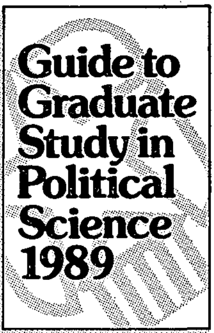

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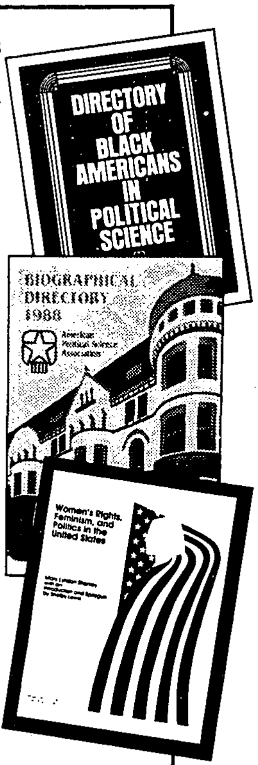
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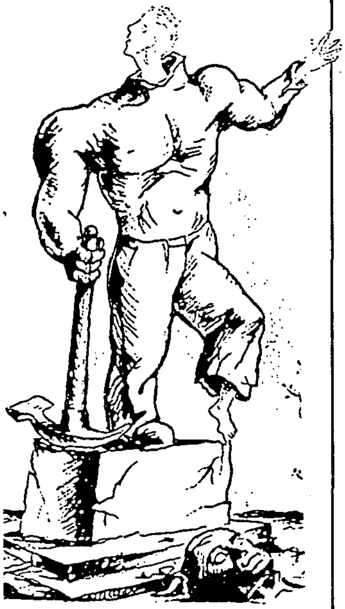

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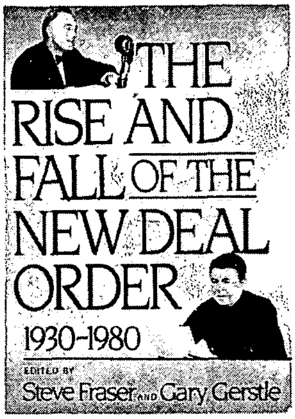
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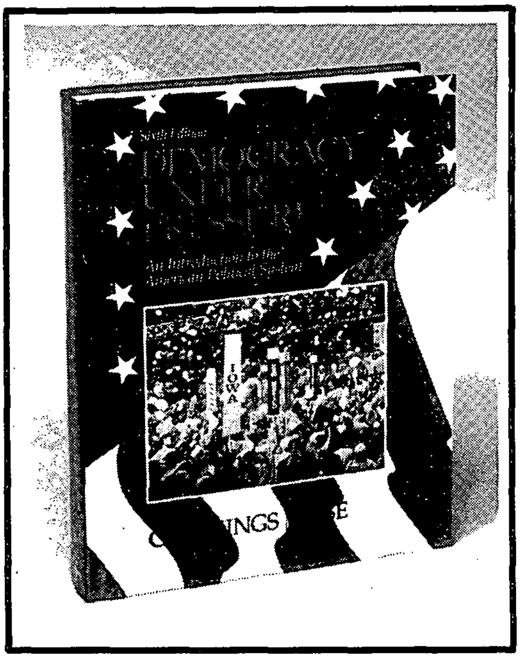

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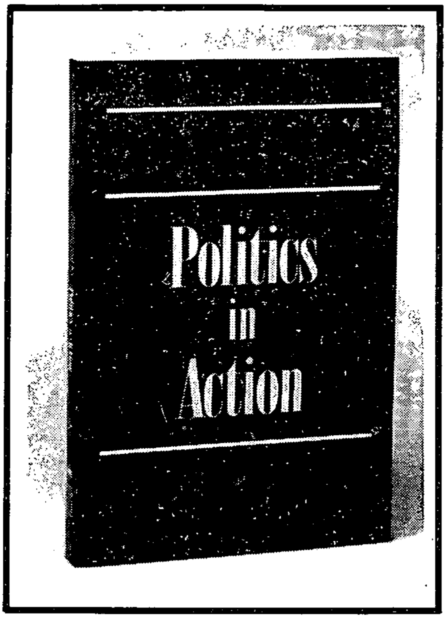
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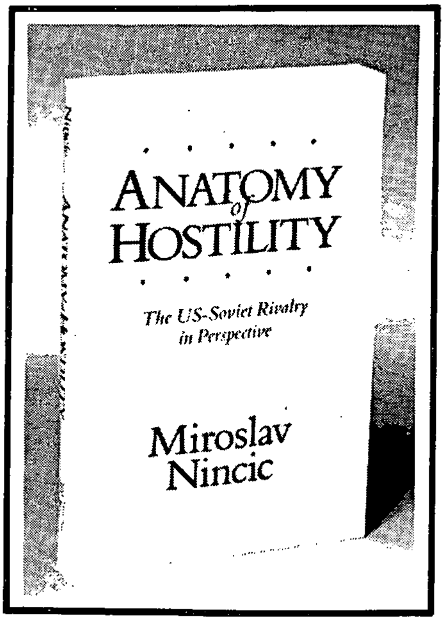
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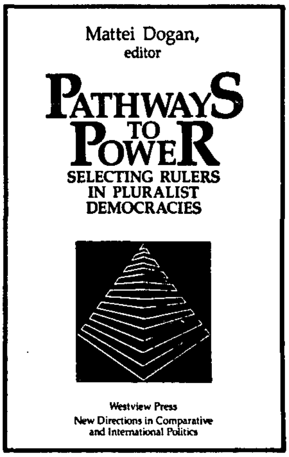

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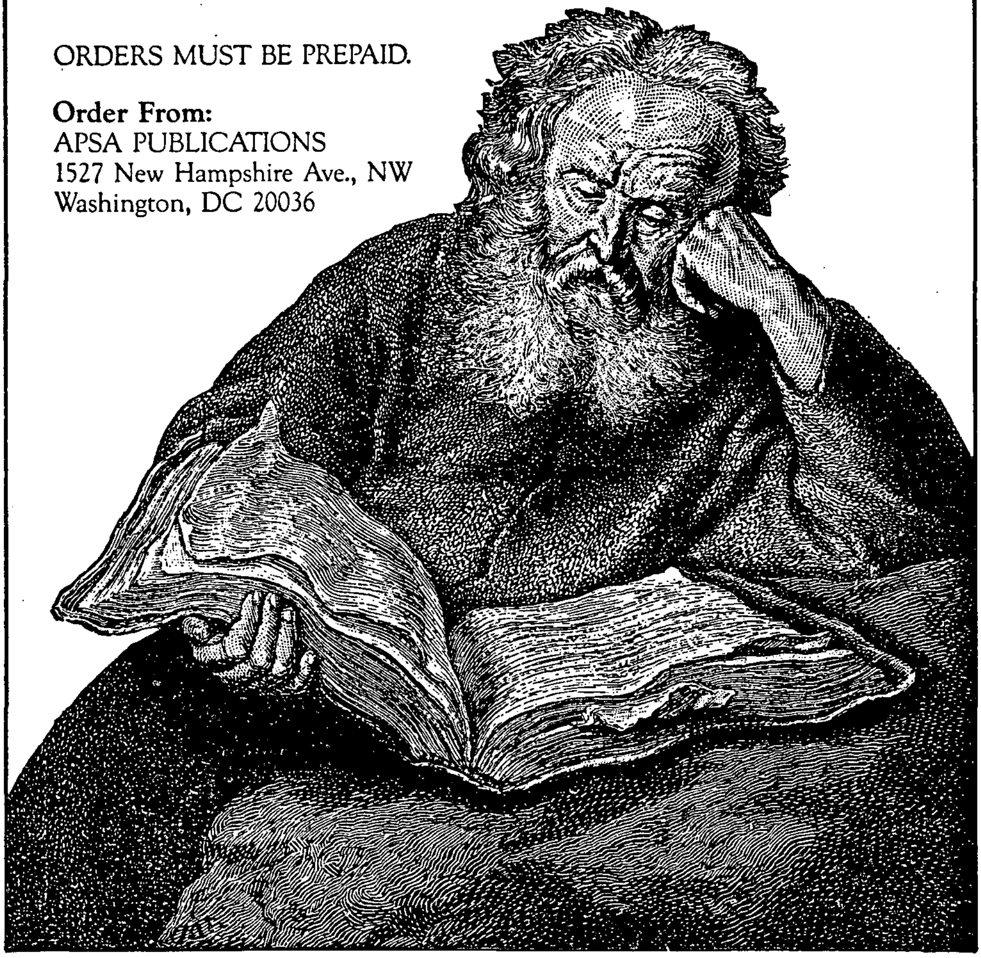

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